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ART. I.—*On the Constitution and Physiology of the Bile.* By JNO. C. DALTON, Jr., M. D., Professor of Physiology and Microscopic Anatomy in the College of Physicians and Surgeons, New York. (With seven wood-cuts.)

NOTWITHSTANDING the readiness with which the bile may be obtained for purposes of examination, the evident importance of the secretion, and the labor which has been bestowed upon it, it must be confessed that we are still very far from having a complete idea of its nature and function as one of the intestinal fluids. The present condition of our knowledge with regard to it, may be briefly summed up as follows: Since the analyses of Strecker, published in 1848 and '49, it has been known that the bile contains, as its essential and characteristic ingredients, two saline substances, the glykocholate and the taurocholate of soda; and that the organic acids of these salts, glykocholic and taurocholic acid, both contain nitrogen; and the latter, in addition to it, two equivalents of sulphur. Besides these peculiar or characteristic ingredients, the bile contains water, a colouring matter (biliverdin), cholesterin, saponifiable and saponified fats, chloride of sodium, earthy and alkaline phosphates, carbonates of soda and potass, and a variable quantity of mucus.

The biliary fluid, thus constituted, was for a long time regarded by many as a simple excretion, like the urine; taking no part in digestion, and destined merely to be expelled from the body. It could not, indeed, be shown to exert any such digestive influence on the alimentary substances, as belonged to the gastric and pancreatic juices; and its loss, when excluded from the alimentary canal, did not give rise to any very marked disturbance, certainly not to a suspension, of the digestive process. But the experiments of Bidder and Schmidt[1] seem to have demonstrated conclusively that its presence in

[1] Verdauungssaefte und Stoffwechsel. Leipzig, 1852.

the alimentary canal is nevertheless essential to the continuance of life; since animals in which the whole of it is drawn off by a biliary fistula, though they still feed and digest well, die after a few weeks, reduced to the last degree of debility and emaciation. What the changes are, however, which it undergoes in the intestine, or in what way it is made subservient to the nutritive functions, has never been definitely ascertained.

Bidder and Schmidt have suggested that the organic acids of the biliary salts were probably decomposed in the intestine, as they may be by boiling with caustic potass in a test tube; giving as the result glycine in the one case, and taurine in the other. But neither of these latter substances has ever been actually found in the intestine. Liebig again suggested that the bile, or at least its essential ingredients, might be reabsorbed from the alimentary canal, to undergo further modifications elsewhere; and Bidder and Schmidt have shown (*op. cit.*) that the feces of the dog do not contain sulphur enough to account for all the (sulphurous) taurocholic acid which is discharged daily with the bile into the intestine. These facts render it exceedingly probable, if not certain, that the bile is actually reabsorbed, under some form or other, from the alimentary canal; but further than that, there is little or nothing definite, with regard to its physiology, to satisfy the mind of the inquirer. All experimenters, who have undertaken the study of this secretion, have found it the most difficult of investigation of all the intestinal fluids; and yet its importance is so palpable, and its occurrence in different species, orders and classes of animals so universal, that it claims, and must continue to receive the special attention of the physiologist.

Within the past two years we have endeavoured to clear up, so far as possible, some of the more obscure points with regard to the history of the bile, and to obtain somewhat more satisfactory notions with regard to its properties and function. The statements which are made in the following pages are derived from the results of sixty-seven different experiments, many of which comprised a series of secondary examinations, and occupied one or two days in their performance. We have examined more particularly the special constitution of the bile in different animals, the best mode of detecting it in intestinal or other fluids, the quantity and time of its discharge into the intestine, its reaction with the gastric and intestinal juices, and lastly its mode of disappearance in the alimentary canal.

Constitution and Chemical Properties of the Bile.—The essential ingredients of ox-bile are, as we have mentioned above, two peculiar saline substances, the glykocholate and the taurocholate of soda. They may be obtained in the following manner: The bile is first evaporated to dryness by the water bath. The dry residue is then pulverized and treated with absolute alcohol, in the proportion of at least ʒj of alcohol to every five grains of dry residue. The filtered alcoholic solution has a clear, yellowish colour. It contains, beside the glykocholate and taurocholate of soda, the colouring matter and more or less of the fats originally present in the bile. On the addition of a small quan-

tity of ether, a dense, whitish precipitate is formed, which disappears again on agitating and thoroughly mixing the fluids. On the repeated addition of ether, the precipitate again falls down, and when the ether has been added in considerable excess, six to twelve times the volume of the alcoholic solution, the precipitate remains permanent, and the whole mixture is filled with a dense, whitish, opaque deposit, consisting of the glykocholate and taurocholate of soda, thrown down under the form of heavy flakes and granules, part of which subside to the bottom of the test tube, while part remain for a time in suspension. Gradually these flakes and granules unite with each other and fuse together into clear, brownish-yellow, oily, or resinous-looking drops. At the bottom of the test-tube, after two or three hours, there is usually collected a nearly homogeneous layer of this deposit, while the remainder continues to adhere to the sides of the glass in small, circular, transparent drops. The deposit is semi-fluid in consistency, and sticky, like Canada balsam or half-melted resin; and it is on this account that the ingredients composing it have been called the "resinous matters" of the bile. They have, however, no real chemical relation with true resinous bodies, since they both contain nitrogen, and differ from resins also in other important particulars.

At the end of twelve to twenty-four hours the glykocholate of soda begins to crystallize. The crystals radiate from various points in the resinous deposit, and shoot up into the supernatant fluid in white silky bundles (Fig. 1). If

Fig. 1.

Fig. 2.

Fig. 1. Ox-bile, extracted with absolute alcohol and precipitated with ether.

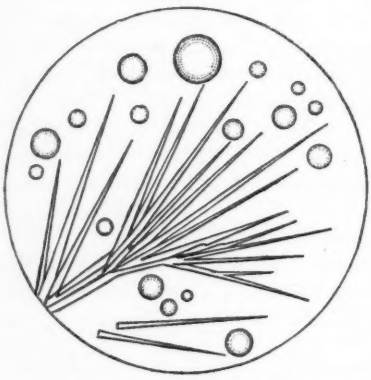
Fig. 2. *Glykocholate of soda, from ox-bile;* after two days' crystallization. At the lower part of the figure the crystals are melting into drops, from the evaporation of the ether and absorption of moisture.

some of these crystals be removed and examined by the microscope, they are found to be of a very delicate acicular form, running to a finely-pointed ex-

tremity, and radiating, as already mentioned, in bundles from a central point (Fig. 2). As the ether evaporates, the crystals absorb moisture from the air, and melt up rapidly into clear resinous drops; so that it is very difficult to keep them under the microscope long enough for a correct drawing and measurement.

The crystallization in the test tube goes on after the first day, and the crystals increase in quantity for three or four, and even five or six days, until the whole of the glykocholate of soda present has assumed the solid form. The taurocholate, however, is uncrystallizable, and remains in an amorphous condition. If a portion of the deposit be now removed and examined by the microscope, it is seen that the crystals of glykocholate of soda have increased considerably in thickness (Fig. 3), so that their transverse diameter may be readi-

Fig. 3.



Glykocholate and taurocholate of soda, from ox-bile; after six days' crystallization. The glykocholate is crystallized; the taurocholate is in fluid drops.

timated. The uncrystallizable taurocholate appears under the form of circular drops, varying considerably in size, clear, transparent, strongly refractive, and bounded by a dark, well defined outline. *They are not to be distinguished, by any of their optical properties, from oil globules* as they usually appear under the microscope. They have the same refractive power, the same dark outline and bright centre, and the same degree of consistency. They would be consequently liable at all times to be mistaken for oil globules, were it not for the complete dissimilarity of their chemical properties.

Both the glykocholate and taurocholate of soda are very freely soluble in water. If the mixture of alcohol and ether be poured off and distilled water added, the deposit dissolves rapidly and completely with a more or less distinct yellowish colour, according to the proportion of colouring matter originally present in the bile. The two biliary substances present in the solution may be separated from each other by the following means. On the addition of acetate of lead, the glykocholate of soda is decomposed, and precipitates as a glykocholate of lead. The precipitate, separated by filtration from the remaining fluid, is then decomposed in turn by carbonate of soda, and the original glykocholate of soda reproduced. The filtered fluid which remains, and which contains the taurocholate of soda, is then treated with subacetate of lead, which precipitates a taurocholate of lead. This is separated by filtration, and decomposed again by carbonate of soda, as in the former case. The two

biliary substances in ox-bile may, therefore, be distinguished by their reactions with the salts of lead. Both are precipitable by the subacetate; but the glykocholate of soda is precipitable also by the acetate, while the taurocholate is not so. If subacetate of lead, therefore, be added to the mixed watery solution of the two substances, and the whole filtered, the subsequent addition of acetate of lead to the filtered fluid will produce no precipitate, because both the biliary matters have been entirely thrown down with the deposit; but, if the acetate of lead be first added, it will precipitate the glykocholate alone, and the taurocholate may afterwards be thrown down separately by the subacetate.

The biliary substances, however, are not the same in different species of animals. In examining the biliary secretions of different species, Strecker found so great a resemblance between them that he was disposed to regard their ingredients as essentially the same. Having established the existence in ox-bile of two peculiar substances, one crystallizable and non-sulphurous (glykocholate), the other uncrystallizable and sulphurous (taurocholate), he was led to consider the bile in all species of animals as containing the same substances, and as differing only in the relative quantity in which the two were present. The only exception to this was supposed to be pig's bile, in which Strecker found a peculiar organic acid, which he called "hyocholic," or "hyocholinic" acid, in combination with soda as a base.

The above conclusion of his, however, was not entirely correct. The bile of all animals, so far as examined, does, it is true, contain peculiar substances which resemble each other in being freely soluble in water, soluble in absolute alcohol, and insoluble in ether; and in giving also a peculiar reaction with Pettenkofer's test, to be described presently. But, at the same time, these substances present minor differences in different animals, which show them not to be identical.

Fig. 4.

Dog's bile, extracted with absolute alcohol and precipitated with ether.

In dog's bile, for example, there are, as in ox-bile, two substances precipitable by ether from their alcoholic solution; one crystallizable, the other not so. But the former of these crystallizes much more readily than the glykocholate of soda from ox-bile. Dog's bile will not unfrequently begin to crystallize freely in five to six hours after precipitation by ether (Fig. 4); while in ox-bile it is usually twelve and often twenty-four and even forty-eight hours before crystallization is fully established. But it is more particularly in their reaction with the salts of lead that the difference between these substances becomes manifest. For, while the crystallizable substance of ox-bile is precipitated by acetate of lead, that of dog's bile is not affected by it. If dog's bile

be evaporated to dryness, extracted with absolute alcohol, the alcoholic solution precipitated by ether, and the ether-precipitate then dissolved in water, the addition of acetate of lead to the watery solution produces not the slightest turbidity. If subacetate of lead be then added in excess, a copious precipitate falls, composed of both the crystallizable and uncrystallizable substances. If the lead-precipitate be then separated by filtration, washed, and decomposed by carbonate of soda, the watery solution will contain the re-formed soda-salts of the bile. The watery solution may then be evaporated to dryness, extracted with absolute alcohol, and the alcoholic solution precipitated by ether, when the ether-precipitate crystallizes partially after a time as in fresh bile. Both the biliary matters of dog's bile are therefore precipitable by subacetate of lead, but neither of them by the acetate. Instead of calling them, consequently, glykocholate and taurocholate of soda, we shall speak of them simply as the "crystalline" and "resinous" biliary substances.

In cat's bile the biliary substances act very much as in dog's bile. The ether-precipitate of the alcoholic solution contains here also a crystalline and a resinous substance, both of which are soluble in water, and both precipitable by the subacetate of lead; but neither of them by the acetate.

In pig's bile, on the other hand, there is no crystallizable substance, but the ether-precipitate is altogether resinous in appearance. Nothwithstanding this, however, its watery solution precipitates abundantly by both the acetate and subacetate of lead.

Fig. 5.

Human bile, extracted with absolute alcohol, and precipitated with ether.

In human bile, again, there is no crystallizable substance. We have found that the dried bile, extracted with absolute alcohol, makes a clear, brandy-red solution, which precipitates abundantly with ether in excess; but the ether-precipitate, if allowed to stand, shows no sign of crystallization, even at the end of three weeks (Fig. 5). If the resinous precipitate be separated by decantation, and dissolved in water, it precipitates, as in the case of pig's bile, by both acetate and subacetate of lead. This might, perhaps, be attributed to the presence of two different substances, as in ox-bile, one precipitated by the acetate, the other by the subacetate of lead. Such, however, is not the case. For if the watery solution be precipitated by the acetate of lead and then filtered, the filtered fluid gives no precipitate afterward by the subacetate; and if first precipitated by the subacetate, it gives no precipitate, after filtration, by the acetate.

Different kinds of bile vary also in other respects, as, for example, their specific gravity, the depth and tinge of colour, the quantity of fat which they contain, &c. &c. Pig's bile is of a nearly clear yellow colour, human bile of a dark golden brown, ox-bile of a greenish yellow, dog's bile of a deep

brown. The alcoholic solution of dried ox-bile does not precipitate at all on the addition of water, while that of human bile, pig's bile, and dog's bile, precipitates abundantly with distilled water, owing to the quantity of fat which it holds in solution. We have found the specific gravity of pig's bile to be 1030 to 1036; that of human bile 1018; that of ox-bile 1024. These variations, however, are of secondary importance in comparison with those which have been already mentioned, and which show that the crystalline and resinous substances in different kinds of bile, though resembling each other in very many respects, are yet in reality by no means identical.

Tests for Bile.—In investigating the physiology of any animal fluid, it is, of course, of the first importance to have a convenient and reliable test by which its presence may be detected. The only test which was for a long time employed in the case of the bile was that which depended *on a change of colour produced by oxidizing substances.* If the bile, for example, or a mixture containing bile, be exposed in an open glass vessel for a few hours, the upper layers of the fluid, which are in contact with the atmosphere, gradually assume a greenish tinge, which becomes deeper with the length of time which elapses, and the quantity of bile existing in the fluid. Nitric acid, added to a mixture of bile and shaken up, produces a dense precipitate which takes a bright grass-green hue. Tincture of iodine produces the same change of colour, when added in small quantity; and probably there are various other substances which would have the same effect. It is by this test that the bile has so often been recognized in the urine, serous effusions, the solid tissues, &c., in cases of jaundice. But it is a very insufficient one for anything like accurate investigation, since the appearances are produced simply by the action of an oxidizing agent on the colouring matter of the bile. A green colour produced by nitric acid does not therefore indicate the presence of the biliary substances proper, but only of the biliverdin. On the other hand, if the colouring matter be absent, the biliary substances themselves cannot be detected by it. For if the biliary substances of dog's bile be precipitated by ether from an alcoholic solution, dissolved in water and decolorized by animal charcoal, the colourless watery solution gives no green colour on the addition of nitric acid or tincture of iodine, though it precipitates abundantly by subacetate of lead, and gives the other reactions of the crystalline and resinous biliary matters in a perfectly distinct manner.

Pettenkofer's Test.—This is undoubtedly the best test yet proposed for the detection of the biliary substances. It consists in mixing with a watery solution of the bile, or of the biliary substances, a little cane sugar, and then adding sulphuric acid to the mixture until a red, lake, or purple colour is produced. A solution may be made of cane sugar, in the proportion of one part sugar to four parts water, and kept for use. One drop of this solution is mixed with the suspected fluid, and the sulphuric acid then immediately added. On first dropping in the sulphuric acid a whitish precipitate falls, which is abundant in the case of ox-bile, less so in that of the dog. This

precipitate redissolves in a slight excess of sulphuric acid, which should then continue to be added until the mixture assumes a somewhat syrupy consistency and an opalescent look, owing to the development of minute bubbles of air. A red colour then begins to show itself at the bottom of the test tube, where the drops of sulphuric acid accumulate, which disappears, however, on agitating the mixture. On continuing the addition of sulphuric acid, the red colour returns and becomes general, till the whole fluid is of a clear bright cherry-red. This gradually changes to a lake colour, and finally to a deep, rich, opaque purple. If three or four volumes of water be then added to the mixture, a copious precipitate falls down, and the colour is destroyed.

Various circumstances modify to some extent the rapidity and distinctness with which the above changes are produced. If the biliary substances be present in large quantity, and nearly pure, the red colour shows itself at once after adding an equal volume of sulphuric acid, and almost immediately passes into a strong purple. If they be scanty, on the other hand, the red colour may not show itself for seven or eight minutes, nor the purple under twenty or twenty-five minutes. If foreign matters, again, not of a biliary nature, be also present, they are apt to be acted upon by the sulphuric acid, and by becoming discoloured interfere with the clearness and brilliancy of the tinges produced. On this account it is indispensable, in delicate examinations, to evaporate the suspected fluid to dryness, extract the dry residue with absolute alcohol, precipitate the alcoholic solution with ether, and dissolve the ether-precipitate in water before applying the test. In this manner all foreign substances likely to do harm will be eliminated, and the test will succeed without difficulty.

It must not be forgotten, beside, that the sugar itself is liable to be acted on and discoloured by sulphuric acid when added in excess, and may therefore by itself give rise to confusion. A little care and practice, however, will enable the experimenter to avoid any chance of deception from this source. When sulphuric acid is mixed with a watery solution containing cane sugar, after it has been added in considerable excess, a yellowish colour begins to show itself, owing to the commencing decomposition of the sugar. This colour gradually deepens until it has become a dark, dingy, muddy brown; but there is never at any time clear red or purple colour unless biliary matters be present. If the bile be present in small quantity the colours produced by it may be modified and obscured by the dingy yellow and brown of the sugar; but even this difficulty may be avoided by paying attention to the following precautions. In the first place, only very little sugar should be added to the suspected fluid. In the second place, the sulphuric acid should be added very gradually, and the mixture closely watched to detect the first changes of colour. If bile be present, the red colour peculiar to it is always produced before the yellowish tinge which indicates the decomposition of the sugar. When the biliary matters, therefore, are present in small quantity, the addition of sulphuric acid should be stopped at that point, and the

colours, though faint, will then remain clear, and give unmistakable evidence of the presence of bile.

The red colour alone is not sufficient as an indication of bile. It is, in fact, only the commencement of the change which indicates the biliary matters. If these matters be present, the colour passes, as we have already mentioned, first into a lake, then into a purple; and it is this lake and purple colour alone which can be regarded as really characteristic of the biliary reaction.

Pettenkofer has given directions, as quoted by Lehmann, that the elevation of temperature in this experiment, naturally produced by mixing sulphuric acid and water, should not be allowed to exceed 120° F. This, however, is not by any means indispensable. We have often found the lake and purple colours to be produced with the greatest intensity, without taking any precaution to keep down the temperature, and while the test-tube was still very hot. Used in this way, Pettenkofer's test may be regarded as of very valuable assistance in the detection of the bile. Its reaction takes place with the bile of all the different species of animals, so far as examined, and with a nearly uniform degree of intensity. It is much more certain and characteristic, therefore, than the test by nitric acid, or tincture of iodine.

Pettenkofer's reaction is produced by the presence of both, or either of the two biliary substances, crystalline or resinous, and is not dependent on the colouring matter of the bile.

1. The bile is evaporated to dryness, the dry residue extracted by absolute alcohol, and the alcoholic solution precipitated by ether. The mixture is then allowed to stand until the crystalline and resinous substances have both completely separated. The mixed alcohol and ether are then poured off, and the precipitate dissolved in distilled water and decolorized. The watery solution now gives Pettenkofer's reaction perfectly, though, as previously mentioned, it does not produce any green colour with nitric acid and a tincture of iodine.

2. The ether-precipitate of the alcoholic solution of dried ox-bile is dissolved in distilled water. The glykocholate of soda is then precipitated from its watery solution by acetate of lead, separated by filtration, washed, re-composed by carbonate of soda, and again dissolved in water. The remainder of the filtered fluid is then precipitated by subacetate of lead, and the precipitate treated as before with carbonate of soda, and dissolved. The two watery solutions, one containing the glykocholate, the other the taurocholate of soda, both give Pettenkofer's reaction decisively and completely.

Various objections have been urged against this test. It has been stated to be uncertain and variable in its action. Robin and Verdeil (*Chimie Anatomique et Physiologique*), say that its reactions "do not belong exclusively to the bile, and may therefore give rise to mistakes." Some fatty substances and volatile oils (olein, oleic acid, oil of turpentine, oil of caraway) have been stated to produce similar red and violet colors when treated with sugar and sulphuric acid. These objections, however, have not much, if any, prac-

tical weight. The test no doubt requires some care and practice in its application, as we have already pointed out; but that is the case, to a greater or less extent, with nearly all chemical tests, particularly those for organic substances. No other substance is liable to be met with in the intestinal fluids or the blood, which would simulate the reactions of the biliary matters. We have found that the fatty matters of the chyle, taken from the thoracic duct, when tried with Pettenkofer's test, do not give any coloration which would be mistaken for that of the bile. When the volatile oils (caraway and turpentine) are acted on by sulphuric acid, a red colour is produced, which afterwards becomes brown and blackish, and a peculiar, tarry, empyreumatic odour is developed at the same time; but we do not get the lake and purple colours spoken of as above. Finally, if the precaution be observed of first extracting the suspected matters with absolute alcohol, then precipitating with ether, and dissolving the precipitate in water, no ambiguity could result from the presence of any of the above substances. The imperfection of the test does not, in fact, consist in its liability to cause other substances to be mistaken for the biliary matters, but in failing sometimes to detect small quantities of the latter when they are really present.

Pettenkofer's test is not a very delicate one.

If two drops of dog's bile be added to ʒj of distilled water, and the mixture tried with Pettenkofer's test, it becomes deep cherry red in half a minute, lake in one minute, and a distinct opaque purple in four minutes.

One drop of dog's bile mixed with ʒj distilled water, tried by the same test, becomes cherry red in two minutes and a half, and lake in four minutes; but there is no purple colour, even at the end of an hour.

One-half a drop of dog's bile with ʒj distilled water, becomes, on the application of Pettenkofer's test, of a somewhat dingy cherry red within a minute; but there is no lake or purple at the end of an hour.

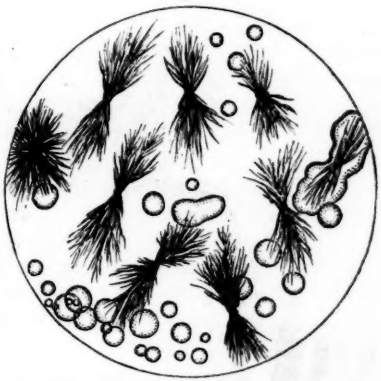
One quarter of a drop of dog's bile with ʒj distilled water, on the addition of sugar and sulphuric acid, becomes immediately yellowish; but afterward only turns of a dingy yellowish brown, hardly, if at all, distinguishable from that produced with a simple solution of cane sugar in water.

Pettenkofer's test, therefore, cannot be relied on for the detection of very minute quantities of the biliary substances. Still it is the best we have, and an admirable one so far as it goes. All chemical tests are limited in this way, with respect to the delicacy of their application. Even Trommer's test for sugar acts very imperfectly with a solution of one-sixteenth of a drop of honey to the drachm of water; and with a solution of one-thirty-second of a drop to the drachm, fails altogether to detect the presence of sugar. Pettenkofer's test, then, if used with care, is extremely useful, and may lead to many valuable results.

With regard to the physiology of the bile, one of the first points which we have endeavoured to examine, is the following: *At what period, and how constantly, is the bile discharged into the intestinal canal?* The experiments

for this purpose were performed on dogs. The animals were kept confined, and killed at various periods after feeding, sometimes by the inoculation of woorara, sometimes by hydrocyanic acid, but most frequently by section of the medulla oblongata. The contents of the intestine were then collected and examined. In all instances the bile was also taken from the gall-bladder, and treated in the same way with the contents of the intestine, for purposes of comparison. The intestinal contents always presented some differences of appearance when treated with alcohol and ether, owing, probably, to the presence of other substances than the bile; but they always gave evidence of the presence of biliary matters as well. The biliary substances could almost always be recognized under the microscope in the ether-precipitate of the alcoholic solution; the resinous substance under the form of rounded, oily-looking drops, and the other under the form of crystalline groups, generally presenting the appearance of double bundles of slender, radiating, slightly curved or wavy needle-shaped crystals. (Fig. 6.) These substances, dissolved in water, gave a purple colour with sugar and sulphuric acid. These experiments were tried after the animals had been kept for one, two, three, five, six, seven, eight, and twelve days without food. The result showed that in all these instances bile was present in the small intestine. It is plainly, therefore, not an intermittent secretion, nor one which is concerned exclusively in the digestive process; but, its secretion is constant and it continues to be discharged into the intestine for many days at least after the animal has been deprived of food.

Fig. 6.

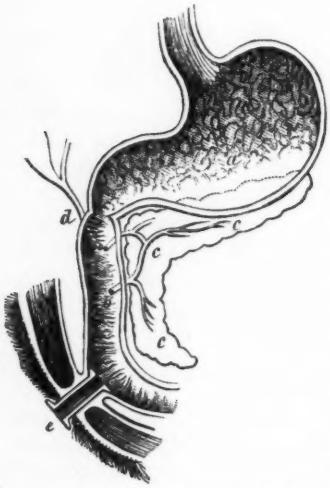


Crystalline and resinous biliary substances; from small intestine of dog after two days' fasting.

The next point of importance to be examined relates to the *time, after feeding, at which the bile passes into the intestine in the greatest abundance.* Bidder and Schmidt (*op. cit.*) have already investigated this point in the following manner. They operated by tying the common bile-duct and then opening the fundus of the gall-bladder so as to produce a biliary fistula by which the whole of the bile was drawn off. By doing this operation, and collecting and weighing the fluid discharged at different periods, they came to the conclusion that the flow of bile began to increase within two and a half to three hours after the introduction of food into the stomach; but that it did not reach its maximum of activity till the end of twelve or fifteen hours. Other observers, however, have obtained different results. Arnold, for ex-

ample (in *Am. Journ. Med. Sci.*, April, 1856), found the quantity to be largest soon after meals, decreasing again after the fourth hour. Kölliker and Müller, again (in *Am. Journ. Med. Sci.*, April, 1857), found it largest between the sixth and eighth hours. Bidder and Schmidt's experiments, indeed, strictly speaking, show only the time at which the bile is most actively secreted by the liver, but not when it is actually discharged into the intestine. Our own experiments, bearing upon this point, were performed on dogs, by making a permanent duodenal fistula on the same plan that gastric fistulæ have so often been established for the examination of the gastric juice (Fig. 7). An incision was made through the abdominal walls, a short distance to the right of the median line, the floating portion of the duodenum drawn up towards the external wound, opened by a longitudinal incision, and a silver tube, armed at each end with a narrow projecting collar or flange, introduced into it by one extremity, five and a half inches below the pylorus, and two and a half inches below the orifice of the lower pancreatic duct. The other extremity of the tube was left projecting from the external opening in the abdominal parietes, the parts secured by sutures, and the wound allowed to heal. After cicatrization was complete, and the animal had entirely recovered his healthy condition and appetite, the intestinal fluids were drawn off at various intervals after feeding, and their contents examined. This operation, which is rather more difficult than that of making a permanent gastric fistula, is nevertheless exceedingly useful when it succeeds, since it enables us to study not only the time and rate of the biliary discharge, but also many other extremely interesting matters connected with intestinal digestion. Of five animals operated on, we lost three, and succeeded in retaining a permanent fistula in two.

Fig. 7.



Duodenal fistula. *a.* Stomach. *b.* Duodenum. *c, c, c.* Pancreas; its two ducts are seen opening into the duodenum, one near the orifice of the biliary duct (*d*), the other a short distance lower down. *e.* Silver tube, passing through the abdominal walls and opening into duodenum.

The results obtained from the experiments on these animals, may be summarily stated as follows. Twenty-four hours after feeding there flows from the fistula a small quantity of fluid, partly brownish and bilious-looking, partly colourless, nearly clear, and more or less frothy. These fluids are mostly neutral, or faintly alkaline, but sometimes have a slightly acid reac-

tion. They come in starts and gushes, sometimes mixed, but very frequently alternating with each other. If the animal be then fed with a full meal (two pounds) of fresh lean meat, during the first fifteen minutes afterward a large quantity of nearly pure bile is poured into the intestine, mingled during the latter part of the time with some gastric juice from the stomach, containing a little albuminose in solution, which precipitates with the bile, forming an opaque bright yellow mixture. This second fluid soon becomes more abundant in proportion to the bile, precipitating a molecular sediment, and becoming less and less strongly coloured. In half an hour to an hour, a fine debris of broken-up muscular fibres begins to pass out of the stomach into the intestine suspended in the gastric juice, forming a grayish, gruelly, fluid mixture, in which the proportion of bile to the other ingredients gradually diminishes. This continues from the second to about the twelfth hour, the proportion of muscular debris growing constantly greater, and that of fluid less, so that the mixture is considerably thicker, and more gruelly than at first. The entire quantity of the mixture, also, grows pretty constantly less until the twelfth hour. After that time, the mixture of muscular debris and gastric juice ceases more or less promptly, and the bile becomes again more abundant in proportion to the other ingredients. It is still mixed, however, with the intestinal fluids, and so continues till the end of the twenty-four hours.

In order to ascertain the absolute quantity of bile discharged into the intestine, and its variations during digestion, the duodenal fluids were drawn off, for fifteen minutes at a time, at various periods after feeding, collected, weighed, and examined separately, as follows: each separate quantity was evaporated to dryness, its dry residue extracted with absolute alcohol, the alcoholic solution precipitated with ether, and the ether-precipitate, regarded as representing the amount of biliary matters present, dried, weighed, and then treated with Pettenkofer's test in order to determine, as nearly as possible, their degree of purity or admixture. The result of these experiments is given in the following table. At the eighteenth hour so small a quantity of fluid was obtained that the amount of its biliary ingredients was not ascertained. It reacted perfectly, however, with Pettenkofer's test, showing that bile was really present.

Time after feeding.	Quantity of fluid in fifteen minutes.	Dry residue of same.	Quantity of biliary matters.	Proportion of biliary matters to dry residue.
Immediately . .	640 grs.	33 grs.	10 grs.	.30
1 hour . . .	1,990 "	105 "	4 "	.03
3 hours . . .	780 "	60 "	4 "	.07
6 hours . . .	750 "	73 "	3½ "	.05
9 hours . . .	860 "	78 "	4½ "	.06
12 hours . . .	325 "	23 "	3¾ "	.16
15 hours . . .	347 "	18 "	4 "	.22
18 hours . . .	—	—	—	—
21 hours . . .	384 "	11 "	1 "	.09
24 hours . . .	163 "	9½ "	3¼ "	.34
25 hours . . .	151 "	5 "	3 "	.60

From this it appears that the bile passes into the intestine in by far the largest quantity immediately after feeding and within the first hour. After that time its discharge remains pretty constant, not varying much from four grains (solid biliary matters) every fifteen minutes, or sixteen grains per hour. (This animal weighed 36½ pounds.)

There is, however, an interval, from about the eighteenth to the twenty-first hour, during which its quantity is much less, and the intestine appears to be in a state of temporary inactivity. But though the absolute quantity of bile remains, with this exception, nearly uniform within the above period, its relative quantity to that of the other ingredients increases pretty constantly after the first hour, owing to the diminishing proportion of the digestive fluids and of the alimentary matters which are undergoing solution.

These facts lead us to the consideration of another question which is of great interest in this connection, viz., *what part, if any, does the bile take in digestion?* We have seen that not only its secretion, but also its discharge into the intestine, are both nearly constant, even after the animal has been many days without food; and consequently that it cannot have, like the gastric and pancreatic juices, an exclusive relation to the digestive process. Still, it is actually present also in the intestine during digestion; and is even discharged into it most abundantly immediately after the introduction of food into the stomach, and before the mixture of gastric juice and half-digested food has begun to pass through the pylorus into the intestine. Is this merely a coincidence, or does it have some definite relation to the changes which the food or the bile or both are to undergo in the alimentary canal? With regard to this question the following facts are of some interest.

The bile precipitates with the gastric juice.

This precipitation can be readily seen in the fluids drawn from the duodenal fistula, half an hour or more after feeding; when the fluids come, as mentioned above, in intermitting starts and gushes, sometimes neutral or alkaline, clear, and brownish, like nearly pure bile, then as a bright, yellowish, turbid mixture, then nearly colourless and acid, consisting mostly of gastric juice with muscular debris. It can also be seen by mixing in a test tube gastric juice from the dog, obtained by means of a gastric fistula, with bile from the gall-bladder of the same animal. If four drops of bile be added in this way to ʒj of gastric juice, a precipitate falls which contains the whole of the colouring matter of the bile; and if the mixture be then filtered, the filtered fluid passes through quite colourless, and is no longer turned green by nitric acid. The gastric juice, notwithstanding this precipitation, retains its acid reaction, but at the same time loses its power of dissolving albuminous substances. For, if gastric juice which has been precipitated in the above manner be filtered, and then kept in a test tube at the temperature of 100° F. with a piece of boiled white of egg, the white of egg becomes somewhat more transparent and brittle and grows contracted and cracked, but the gastric juice exerts little or no dissolving action upon it. This reaction of

the bile and gastric juice deserves a closer examination, since the two fluids certainly do mix in the intestine, and must exert a more or less important action on each other.

It is the *biliary substances themselves which cause the precipitation with gastric juice.*

If the crystalline and resinous substances of dog's bile be separated from the other ingredients by the process already several times described, and after precipitation by ether dissolved in distilled water, they make a clear, colourless solution. Such a solution, made in the proportion of three grains biliary matters to ℥j of water, precipitates with gastric juice like the bile from the gall-bladder; only the precipitate in this instance is colourless. When fresh bile, therefore, is used, the colouring matter is merely entangled and thrown down with some other substance; the precipitate takes place, however, with the biliary matters proper just as well when the colouring matter is absent.

But *the biliary substances themselves are not precipitated;* for if ℥j of filtered gastric juice, taken from the stomach six hours after feeding, be precipitated by the addition of four drops, two drops, or even one drop of bile from the gall-bladder, and the turbid mixture filtered, the clear filtered fluid in every instance gives abundant evidence of the presence of biliary matters by Pettenkofer's test. The biliary substances, therefore, or at least by far the greater part of them, remain in solution and do not fall down with the precipitate by gastric juice.

This reaction of the biliary and gastric fluids, though very important in respect to the theory of intestinal digestion, does not appear to have any particular bearing on the subsequent history of the bile itself. It is the gastric juice and the alimentary substances which are affected by coming in contact with the bile, not the biliary ingredients themselves. The effect of this precipitation, however, on the gastric juice and food, even, is not very thoroughly understood. It has been thought by Bernard that the contact of the bile stops altogether the digestive action of the gastric juice, precipitating at the same time all the albuminose which it had previously dissolved; this precipitated albuminose being afterward dissolved by the action of the pancreatic juice, which is regarded by M. Bernard as an exceedingly active agent in intestinal digestion. Some facts which have been mentioned above, seem, indeed, to support this opinion; as, for example, that the gastric juice, when precipitated with bile, loses its power of artificially dissolving boiled white of egg in a test tube. But it very soon becomes evident to the experimenter that these artificial digestions do not always represent exactly the process as it takes place in the living animal. They may be of great service in suggesting and directing subsequent examinations, but can rarely be depended on exclusively as settling any given question. The gastric fluids, taken from the stomach some time after feeding and filtered, often contain organic matter in solution under a different form from that which it assumes when the digestion is conducted artificially in test tubes. We have found, in

point of fact, that bile from the dog's gall-bladder always precipitates with the gastric juice as it passes from the stomach into the intestine, whether it be taken from the stomach fifteen minutes, half an hour, or six hours after feeding. The acid fluids drawn from the duodenal fistula, also, three and six hours after feeding, will precipitate with those drawn twenty to twenty-five minutes after feeding, and in which the proportion of biliary matters is larger.

But there are some considerations which militate against the simple view of intestinal digestion entertained by M. Bernard. In the first place the gastric fluids precipitate with the pancreatic juice as well as with the bile, when mixed in a test tube; while the two intestinal fluids, bile and pancreatic juice, do not precipitate with each other. In the second place, it is a remarkable fact that bile very constantly *finds its way into the stomach, in larger or smaller quantities, at almost all periods of digestion.* This fact has already been noticed by Lehmann, who states (*Physiological Chemistry*, Philad. ed., vol. i. p. 447) that he has "made few examinations of human bodies, or even of recently killed healthy animals, in which he has not discovered biliary constituents in the contents of the stomach lying near the pyloric end." In the pig it is almost universal to find the pyloric portion of the gastric mucous membrane after death permanently stained of a bright yellow by bile. It is very common indeed, furthermore, while drawing off the fluids of the stomach by a gastric fistula within the first fifteen minutes after feeding, to see bile suddenly make its appearance, evidently by regurgitating through the pylorus, when the clear, colourless gastric juice instantly becomes turbid and yellowish. In a few moments the bile may cease to present itself, and the gastric fluids regain their colourless and transparent appearance, to be again, perhaps, rendered turbid and yellow some moments afterward. The gastric fluids, even, which are drawn six hours after feeding, and which are thick, grayish and gruelly in appearance, if filtered clear, will frequently give distinct evidence of the presence of bile by Pettenkofer's test. All this certainly does not essentially interfere with the process of gastric digestion; and the action of the bile on the digested food in the intestine evidently does not correspond exactly with any explanation which has been suggested.

The next series of experiments to which we resorted were undertaken in order to investigate, so far as possible, the following very obscure question, viz: *What becomes of the bile in its passage through the intestine?* The dogs used for these experiments were fed with fresh meat and then killed at various intervals after the meals, the abdomen opened, ligatures placed upon the intestines at different points, and the contents of their upper, middle and lower portions collected and examined separately. The results thus obtained show that, under ordinary circumstances, the bile, which is quite abundant in the duodenum and upper part of the small intestine, diminishes in quantity from above downward, and is not to be found in the large intestine. The entire quantity of the intestinal contents diminishes and their consistency increases as we approach the ileo-cæcal valve. At the same time their colour

changes from a light yellow to a dark bronze, or blackish green, which is always strongly pronounced in the last quarter of the small intestine. The following experiment will serve to show the plan which was followed and its results.

Experiment.—A full-grown, healthy bitch, was fed with fresh meat, then kept entirely without food, but supplied only with water, for five days. At the end of that time she was killed by section of the medulla.

The stomach contained fl℥ss of a nearly colourless, dingy, frothy, neutral fluid.

Upper half of small intestine contained 117 grains of a dull yellow, gelatinous, semi-fluid, neutral matter. Its dry residue was 24 grains.

Lower half of small intestine contained 130 grains of a much darker bronze-coloured neutral gelatinous mass, a large proportion of which consisted of hairs and intestinal worms, which were not present in the contents of upper half. Dry residue 20 grains.

Large intestine contained 210 grains of a dark, olive-brown, consistent mass, slightly acid in reaction. Dry residue 73 grains.

All three lots were then evaporated to dryness, and afterward exhausted by repeated extraction with absolute alcohol, until the filtered alcohol came through perfectly colourless, and no longer gave any turbidity with ether. The alcoholic solutions were then reduced by evaporation to the same volume and precipitated with ether in excess. The ether-precipitate was separated, dried under the air-pump, and weighed. The results obtained in this way are given in the two following tables:—

TABLE 1.

	Weight of fresh contents.	Dry residue of same.	Ether-precipitate.	Proportion of ether-precip. to fresh contents.
Upper half of small intestine	117 grains.	24 grains.	5 grains.	.0427
Lower " "	130 "	20 "	5 "	.0384
Large intestine . . .	210 "	73 "	3 "	.0142

TABLE 2.

	Weight of solid residue.	Ether-precipitate.	Proportion of ether-precip. to solid residue.
Entire small intestine . .	44 grains.	10 grains.	.227
Large intestine	73 "	3 "	.041

The ether-precipitate of the alcoholic solution is, therefore, both positively and relatively very much less in the large intestine than in the small. Its proportion to the entire solid contents is only one-fifth or one-sixth as great in the large as it is in the small intestine. But even this small quantity does not consist of biliary matters; for the dried ether precipitates, in the above experiment, when dissolved in distilled water all three precipitated by sub-acetate of lead; but that from the large intestine very much the least. The watery solutions being treated with sugar and sulphuric acid, those from both portions of the small intestine gave Pettenkofer's reaction promptly and perfectly in less than a minute and a half; while in that from the large intestine

no red or purple colour was produced even at the end of three hours, but only a dingy, muddy brown.

The small intestine, consequently, contains at all times substances giving all the reactions of the biliary ingredients; while in the contents of the large intestine no such substances can be recognized by Pettenkofer's test.

The biliary matters, therefore, disappear in their passage through the intestine. This disappearance may be explained in two different ways. First, the biliary matters may be actually reabsorbed from the intestine, and taken up by the bloodvessels; or second, they may become so altered and decomposed by the intestinal fluids as to lose the power of giving Pettenkofer's reaction with sugar and sulphuric acid, and pass off with the feces in an insoluble form. The first of these explanations is that which has recently been regarded with the most favour; and it is, in fact, rendered extremely probable by the experiments of Bidder and Schmidt, already referred to, in which they found that the entire quantity of sulphur contained in the feces of the dog during five days was very much less than that which must have been discharged with the bile into the intestine during the same time. It is almost impossible to avoid the conclusion that it has been reabsorbed by the bloodvessels. Still this gives us no idea how far the bile is altered before its reabsorption; and the direct and absolute proof, also, of finding the biliary matters in the blood of the portal vein is still wanting. We have endeavoured to supply the latter deficiency by examining the portal blood in dogs, killed at various times after feeding. The animals were killed by section of the medulla oblongata, a ligature immediately placed on the portal vein, while the circulation was still active, and the requisite quantity of blood collected. The blood was sometimes immediately evaporated to dryness by the water bath. Sometimes it was coagulated by boiling in a porcelain capsule over a spirit lamp, with water and an excess of sulphate of soda, and the filtered watery solution afterward examined. But most frequently, the blood, after being collected from the vein, was coagulated by the gradual addition of three times its volume of alcohol at ninety-five per cent., stirring the mixture constantly, so as to make the coagulation gradual and uniform. It was then filtered, the moist mass remaining on the filter subjected to strong pressure in a linen bag by a porcelain press, and the fluid thus obtained added to that previously filtered. The entire spirituous solution was then evaporated to dryness, the dry residue extracted with absolute alcohol, and the alcoholic solution treated as usual to discover the presence of biliary matters. In every instance blood was taken at the same time from the jugulars or the abdominal vena cava, and treated in the same way for purpose of comparison. We have examined the blood in this way one, four, six, nine, eleven and a half, twelve and twenty hours after feeding. As the result of these examinations it was found that in the venous blood, both of the portal vein and of the general circulation, there exists a substance soluble in water and absolute alcohol, and precipitable by ether from its alcoholic solution. This substance is often considerably more abundant in the portal blood than in that from the general

system. It adheres closely to the sides of the glass after precipitation, so that it is always difficult and often impossible to obtain enough of it mixed with ether for microscopic examination. It dissolves also, like the biliary substances, with great readiness in water; but in no instance have we ever been able to obtain from it such a satisfactory reaction with Pettenkofer's test, as would indicate the presence of bile. This is not because the reaction is masked, as might be suspected, by some of the other ingredients of the blood; for if at the same time two drops of bile be added to half an ounce of blood taken from the abdominal vena cava, and the two specimens treated alike, the ether precipitate may be considerably most abundant in the case of the portal blood; and yet that from the blood of the vena cava, dissolved in water, will give Pettenkofer's reaction for bile perfectly, while that of the portal blood will give no such reaction. Notwithstanding, then, the strong probability that the biliary matters are taken up by the portal blood, we have failed to recognize them there by Pettenkofer's test.

From the facts, therefore, which have been detailed above, we may derive the following conclusions:—

I. The two biliary substances, crystalline and resinous, are not the same in different species of animals, though they resemble each other in most of their chemical properties.

II. In all cases, they act in the same way with Pettenkofer's test, whether both or only one of them be present.

III. In different kinds of bile, the biliary matters are to be distinguished from each other principally by their reaction with the salts of lead.

IV. In human bile there is no crystallizable biliary substance, but only a resinous one. The same thing is the case with the bile of the pig.

V. Pettenkofer's reaction is the only available test for the biliary substances proper. It may fail to detect them when present in very small quantity; but, if used with care, will not lead us to mistake other substances for them.

VI. The bile in the carnivorous animals passes into the intestine for at least twelve days after the last meal.

VII. It is discharged into the intestine most abundantly immediately after feeding; during the remainder of the twenty-four hours its flow is about uniform (sixteen grains biliary matters per hour in a medium sized dog), except from about the eighteenth to the twenty-first hour, during which time it is much less.

VIII. When the bile comes in contact with the gastric fluids, the organic matters of the latter are precipitated; but the biliary substances remain in solution.

IX. The biliary substances disappear during their passage through the intestine, so that they can no longer be recognized by Pettenkofer's test.

X. They are, in all probability, reabsorbed into the blood; but if so, they first undergo in the intestine such changes, that they no longer give Pettenkofer's reaction with sugar and sulphuric acid.

ART. II.—*Compound Dislocation of the Long Bones; considered with Especial Reference to the Value of Resection.* By FRANK HASTINGS HAMILTON, M. D., Prof. of Surgery in the Med. Dep. University of Buffalo.

Frequency of Compound as compared with Simple Dislocations.—Compound dislocations, as compared with simple, are of rare occurrence. Of ninety-four dislocations reported by Norris as having been received into the Pennsylvania Hospital for the ten years ending in 1840, only two were compound;[1] and of one hundred and sixty-six dislocations recorded in my observations, only eight were compound.[2]

Relative Frequency in the Different Joints.—In my own recorded cases, four were dislocations of the tibia inward at the ankle-joint, one was a partial (pathological) luxation forward at the same joint, one was a luxation of the astragalus, one a luxation of the head of the humerus into the axilla, and one a forward luxation of the radius and ulna at the wrist-joint. Both of the cases reported by Norris were dislocations of the thumb.

Sir Astley Cooper, speaking upon this point,[3] says that the elbow, wrist, ankle, and finger-joints are most subject to these accidents; and that he has seen but two in the shoulder-joint, and one in the knee-joint. He had never seen a compound dislocation at the hip-joint, and he believed that it was "scarcely ever" so dislocated. On p. 119, however, Mr. Bransby Cooper has reported in detail a very interesting case of this accident, communicated to him by Dr. Walker, of Charlestown, Mass., in which reduction was accomplished by *manipulation* alone, by Dr. Ingalls, on the second day. The patient died at the end of about three weeks. So far as I know, this is the only case upon record. Malgaigne says that a compound dislocation at the hip-joint has probably never occurred.[4]

Among the cases of compound dislocation recorded by Sir Astley and Bransby Cooper, most of which were communicated to these gentlemen by other surgeons, 45 were dislocations of the ankle, 10 of the astragalus, 4 of the ulna at the wrist-joint, 4 of the thumb, 2 of the knee, 1 of the shoulder, 1 of the elbow, 1 of the radius and ulna at the wrist, 1 of the scaphoid bone, and 1 of the metatarsal bone of the great toe. Other writers have occasionally described compound dislocations of the clavicle, but I know of no record of a compound dislocation of the lower jaw.

[1] See this Journal, vol. xxvii. p. 335 (1841).

[2] For the most of these cases, see Transactions of the New York State Med. Soc. for 1855; article entitled "Report on Dislocations, with Especial Reference to their Results." By Frank H. Hamilton.

[3] Treatise on Disloc., &c., Amer. ed., 1851, p. 59.

[4] Traité des Frac. et des Lux., tom. ii. p. 212.

Prognosis, as determined by the Mode of Treatment adopted by most of the Ancient and many of the Modern Surgeons.—By most of the early writers these accidents, whenever they occurred in the larger joints, were regarded as nearly beyond the reach of art. Says Hippocrates:—

"In cases of complete dislocation at the ankle-joint, complicated with an external wound, whether the displacement be inwards or outwards, you are not to reduce the parts, but let any other physician reduce them if he choose. For this you should know for certain, that the patient will die if the parts are allowed to remain reduced, and that he will not survive more than a few days, for few of them pass the seventh day, being cut off by convulsions, and sometimes the leg and foot are seized with gangrene."—*Works of Hippocrates, published by the Sydenham Soc., London*, vol. ii. p. 634. Hippocrates adds, "But if not reduced, nor any attempts at first made to reduce them, most of such cases recover."—*Op. cit.*, p. 634.

The same remarks are applied by Hippocrates to compound dislocations of the head of the tibia, of the lower end of the femur, of the wrist, elbow, and shoulder-joints; death occurring in all cases, as he believes, more or less speedily whenever the bones are reduced and retained in place a sufficient length of time, and "were it not that the physician would be exposed to censure" (*op. cit.*, p. 638), he would not reduce even the bones of the fingers, since it must be expected, he thinks, that their articular extremities will exfoliate even when the reduction is most successful.

I shall presently show, however, that even Hippocrates advised and probably practised resection in certain cases of these accidents.

Both Celsus and Galen adopt almost without qualification the line of practice laid down by Hippocrates, and affirm equally the danger and almost certain death consequent upon the reduction of compound dislocations in large joints.[1] Celsus recommends resection in some cases.

Paulus Ægineta, however, and after him Albucasis, Haly Abbas and Rhazes, do not regard the rules established by Hippocrates in relation to the non-reduction of the bones as so imperative, nor the results of the opposite practice as so uniformly fatal.

"Hippocrates remarks," says Paulus Ægineta, "in the case of dislocations with a wound the utmost discretion is required. For these, if reduced, occasion the most imminent danger, and sometimes death, the surrounding nerves and muscles being inflamed by the extension, so that strong pains, spasms, and acute fevers are produced more particularly in the case of the elbows, knees and joints above, for the nearer they are to the vital parts the greater is the danger they induce. Wherefore, Hippocrates, by all means, forbids us to apply reduction and strong bandaging to them, and directs us to use only anti-inflammatory and soothing applications to them at the commencement, for that by this treatment life may sometimes be preserved. But what he recommends for the fingers alone we would attempt to do for all the other joints; at first, and while the parts remain free from inflammation, we would reduce the dislocated joint by moderate extension, and if we succeed in our object we may persist in using the anti-inflammatory treatment only. But if inflammation, spasm, or any of the afore-mentioned symptoms come on, we must dislocate it again if it can be done without violence. If, however, we

[1] Paulus Ægineta, vol. ii. p. 510, Syd. ed.

are apprehensive of this danger (for perhaps if inflammation should come on it will not yield), it will be better to defer the reduction of the greater joints at the commencement; and when the inflammation subsides, which happens about the seventh or ninth day, then, having foretold the danger from reduction, and explained how, if not reduced, they will be mutilated for life, we may try to make the attempt without violence, using also the lever to facilitate the process."[1]

In the following quotations from three of the most celebrated writers of the two last centuries we find but little if any evidence that the opinions of the fathers upon this subject were not still held in general respect:—

"If the joint be dislocated, so that it is either uncovered, or a little thrust forth without the skin, the accident is mortal, and of more danger to be reduced than if it be not reduced. For if it be not reduced, inflammation will come upon it, convulsion, and sometimes death. 2. There will be a filthiness of the part itself. 3. An incurable ulcer, and if perhaps it be brought to cicatrize at all, it will easily be dissolved by reason of the softness of it; but if it be reduced, it brings extreme danger of convulsion, gangrene, and death."[2]

"Si vero in magnis articulis tam valida fuit facta luxatio, ut ligamentis ruptis os articuli multum sit protrusum per integumenta, hæc pars ossis vasis privata moritur, citius autem si reponatur, quam si non reponitur; quare sola amputatio restat ad conservationem vitæ."[3]

Heister, who makes no allusion to this subject in the first edition of his great work, published at Amsterdam in 1739, adds the following remarks in his last edition, translated and published in London in 1768:—

"Dislocations attended with a wound, especially of the shoulder or thigh bone, are of very bad consequence, and often endanger the life of the patient; in Celsus's opinion (Book VIII. Chap. XXV.), whether the bones be replaced or not, there is generally great danger; and so much the more the nearer the wound is to the joint. Hippocrates has declared that no bones can be reduced with security, beside those of the hands and feet (*Vectiar.* 19, 5). See more on this subject in that passage of Celsus just now quoted, though I by no means recommend the following him implicitly."[4]

Such were the extreme views as to the fatality of these accidents, and of the feebleness of our resources entertained by the ancient, and even by the more modern writers almost down to our own day; with only rare exceptions these limbs were condemned either to great and inevitable deformity, or to amputation. Nor, if we speak only of their fatality, have surgeons ceased to regard these accidents as among the most grave with which they have to deal.

Pathology and Appreciation of the Sources of Danger, as compared especially with Compound Fractures.—The danger, according to Sir Astley Cooper, consists in the rapid inflammation of the synovial membranes, which

[1] Works of Paulus Ægineta, Sydenham ed., vol. ii. p. 509.

[2] "Chirurgeon's Storehouse." By Johannes Scultetus, of Ulme, in Suevia. London ed., 1674, p. 31.

[3] Johannes de Gorter. Chirurgia repurgata. Lugduni Batavorum, 1742. p. 86.

[4] General System of Surgery, by Dr. Laurence Heister. 8th ed. London, 1768. Vol. i. p. 164.

is speedily followed by suppuration and ulceration, whereby the ends of the bones become exposed; and for the repair of which lesions great general as well as local efforts are required, and a high degree of constitutional irritation results. In addition to which circumstances, "the violence inflicted on the neighbouring parts, the injury of the muscles and tendons, and the laceration of bloodvessels, necessarily lead to more important and dangerous consequences than those which follow simple dislocations."[1]

The sources of danger enumerated by Sir Astley Cooper have been regarded as sufficient to account for their extraordinary fatality by the majority of those modern surgical writers who have alluded to the subject; but I must confess that to me they do not appear so. In compound fractures the mortality is far less; yet one might naturally suppose, that when the sharp and irregular fragments are pressing into the flesh, among nerves and bloodvessels, the irritation and inflammation would be equal, if not more than equal to the irritation and consequent inflammation produced by exposing a joint surface to the air; indeed, modern experience has sufficiently shown that these surfaces are much more tolerant of atmospheric exposure, and of the action of many other irritants, than surgeons formerly supposed. A clean incision into a large joint, which exposes the synovial membranes to the air, and which permits the products of inflammation to escape freely, is attended with much less danger than a small puncture which does not at all permit the air to enter, nor the increased synovia and the pus to escape. Very grave results sometimes follow from large wounds into large joints, but under judicious treatment such results are the exception and not the rule.[2] But Sir Astley evidently attributes more of the bad consequences to the exhausting effects of the efforts at repair, than to the immediate inflammation resulting from the exposure of the joint. It is pretty certain, however, that a majority of these patients die at a period too early to render this cause in any considerable degree operative.

As to the bruising of the "muscles and tendons, and laceration of bloodvessels," it cannot be denied that it must usually be greater than in "simple dislocations;" and I will not say that it is not in a given number of instances greater than in the same number of instances of compound fractures. The tissues have often been thrust rudely through by a large and smooth bone, and the tendons have been stretched violently or torn completely asunder; while occasionally large arteries, which are prone to hug the bones about the joints, are lacerated and left to bleed. That the importance of these complications, however, may not be over-estimated, we must state that Sir Astley Cooper himself has remarked how seldom, in compound dislocations of the

[1] On Dislocations and Fractures. Amer. ed., 1851, p. 59.

[2] Upon this point see the very able article entitled "Amputations and Compound Fractures," by John O. Stone, in the New York Journal of Medicine, vol. iii. of 2d series, p. 316. Nov. 1849.

ankle-joint, the large arteries are injured; that a tearing of the ligaments and of the tendons is almost as likely to occur in simple dislocations as in compound; and, indeed, that in neither case are the tendons usually ruptured, but only thrust aside. Moreover, the skin is often made to give way not so much from the pressure of the round head within, as from the equal pressure of some sharp angular body from without. In all of these respects, there are many examples of compound fractures which possess not a whit of advantage; in which cases, nevertheless, the surgeon feels very little doubt as to the ultimate cure.

In short, the causes which, according to Sir Astley Cooper, determine the extraordinary fatality of these accidents, do not sufficiently differ from those which operate in compound fractures to occasion so great a difference in results, and the fatality of compound dislocations remains unexplained; or if surgical writers have here and there intimated the true cause, they have failed to give it its proper place and value.

I think the cause of the greater fatality of compound dislocations over compound fractures is to be found in the simple fact that dislocations are generally reduced, and by splints or other apparatus successfully maintained in place, while compound fractures, as my reports of cases have proven, are not generally reduced completely, nor can they by any means yet devised, except in a few cases, be maintained in place if reduced. Broken limbs, whether simple or compound in their character, will in a great majority of cases shorten upon themselves in spite of the most assiduous and skilful attempts to prevent it.

In adults most bones break obliquely, and cannot be made to support each other, and even in transverse fractures the broken ends are generally small compared with the articular ends of the same bones, and afford a very uncertain and inadequate support for themselves; not to speak of the difficulty of once bringing their ends into exact apposition where the muscles are powerful, or they lie embedded in a large mass of flesh, so that they cannot be felt. While, on the other hand, dislocated bones, whether simple or compound, are capable when restored to place of supporting themselves; or with only slight assistance, their reduction may be maintained: it is also ordinarily a work of no great difficulty to reduce them.

Herein, then, consists the most important difference between these two classes of accidents, which are in other respects so similar. In the one, the very nature of the injury prevents the complete reduction, and the consequent violent strain of the muscles, tendons, and other soft tissues; while in the other, the nature of the accident leaves it in the power of the surgeon to reduce the bones, and modern surgery has in a great measure sanctioned the practice of maintaining them in place, in defiance of the efforts of the muscles to shorten the limb, and probably to the imminent hazard of the life of the patient.

Is it not fair to presume that tissues which have been lacerated and

stretched, require rest in order that they may recover from the effects of their injuries? And if the soft parts are really more injured in dislocations than in fractures, does not the indication for rest become, for this very reason, more imperative?

General Inferences.—We have come, then, to regard the shortening of limbs after fractures, within certain limits and in certain cases, as a conservative circumstance rather than as a circumstance which the surgeon should in all cases seek to prevent.

There is abundant evidence that the ancients had some knowledge of the value of rest to the muscles, tendons, &c., in the prevention of inflammation after compound dislocations, since they constantly urge the greater danger of reducing these dislocations, than of leaving them unreduced; and they do not hesitate to recommend, that in case violent inflammation supervenes upon the reduction, the bone shall immediately be again dislocated. Galen speaks very explicitly on this subject, and says that "the danger in reduction consists partly in the additional violence inflicted on the muscles, and partly in their being then put into a stretched state, whereby spasms or convulsions are brought on, and gangrene as the result of the intense inflammation which ensues;"[1] and Paulus Ægineta remarks: "For these, if reduced, occasion the most imminent danger, and sometimes death; the surrounding nerves and muscles being inflamed by the extension," &c.[2]

I have already quoted from Sir Astley Cooper the causes to which he attributes in general the great fatality of compound dislocations; and the same reasons have generally been assigned by those who have written since his day; but he has elsewhere, when speaking of exsection, given place to the very idea for which we claim so much prominence, the danger arising from a stretching of the muscles.[3] Mr. Liston,[4] also, and Mr. Miller,[5] when speaking especially of dislocations of the tibia at the ankle-joint, refer to the same source of danger.

Treatment.—Let us see now the alternatives which surgery presents for the treatment of these intractable accidents.

1. Reduction of the bone.
2. Non-reduction.
3. Amputation.
4. Tenotomy.
5. Resection and reduction.

The questions for us to consider are, first, by which of these several methods is the life of the patient rendered most secure; and second, where of two or

[1] Works of Hippoc., vol. ii. p. 634. Note by the translator.
[2] Paulus Ægineta, vol. ii. p. 509.
[3] Cooper on Disloc. and Frac., Amer. ed., 1851, p. 270.
[4] Liston's Practical Surgery, Amer. ed., p. 97.
[5] Miller's Principles of Surgery, Amer. ed., p. 684.

more methods all are equally safe, by which will he suffer the least maiming or mutilation.

By Reduction.—We have seen already how the old surgeons regarded the practice of reducing compound dislocations of the larger joints. It is not difficult, however, to find in the records of surgery numerous examples of their successful termination under this practice.

In the third vol. of this journal, p. 109, may be found a case of this kind, in which the dislocation was at the ankle-joint, reported by Dr. White, of Hudson, N. Y. Pott says he has seen this practice occasionally succeed,[1] and Mr. Scott communicated to the *Lancet* in March, 1837, a case of compound dislocation of the humerus successfully treated by reduction. Sir Astley Cooper also records several cases of compound dislocations at the lower end of the tibia and fibula, successfully treated by reduction.

A careful examination, however, of those cases reported by Sir Astley as having been reduced without resection, and which resulted in cures, does not, in my opinion, leave much substantial evidence in favour of the practice; or perhaps we ought rather to say that it leaves only a qualified evidence of its propriety in certain cases. He has mentioned about sixteen of these examples, comprising dislocations of the lower end of the tibia, or of the tibia and fibula outwards and inwards and forwards, all of which, save one quoted from Mr. Liston, have been reported to him by other surgeons, and not one of which he had ever seen himself. Many of the cases are reported very loosely, evidently in reply to circular letters, and from memory, without recorded notes, and by unknown, and in some sense irresponsible surgeons. It is not always said whether the wounds in the soft parts were made by the protrusion of the bones, or by some external violence; yet this is certainly a very material point in determining whether reduction is to be followed by inflammation or not. The results, sometimes only accomplished after exposure to great hazards, are, after all, often sufficiently unfavourable.

It will be noticed, also, that in Cases 152 and 153, the astragalus was comminuted and removed, either at first or at a later day; and in Cases 154, 155, 156 and 160, the tibia, and also probably the fibula, was broken, and it does not appear but that in consequence of this complication the limb became shortened, and the muscles were thus put at rest, very much as if the bones had been retracted; and in one of the cases enumerated under 161, the lower end of the tibia spontaneously exfoliated. That a comminution, or that any fracture of the astragalus or of the tibia and fibula, should be regarded in these cases as rendering the accident less grave, can only be comprehended by a full appreciation of the value of relaxation of the muscles.

The few cases which remain after this exclusion do indeed illustrate how nature and skill may triumph over great difficulties, but nothing more.

Non-Reduction.—While, on the other hand, it will be very difficult to find an equal number of cases of compound dislocations unreduced, which have

[1] Chirurg. Works, vol. ii. p. 243.

terminated favourably, surgeons who have themselves left such limbs unreduced, would scarcely be willing to give them the notoriety of a public record. In the *Transactions* of the New York State Medical Society for 1855, I have reported (Case 16 of Tibia and Fibula, p. 87), a compound disclocation at the ankle-joint, which, being unreduced, terminated fatally on the twenty-eighth day. This is the only example of a compound dislocation of a long bone, left unreduced, which has fallen under my observation; excepting, of course, those cases in which amputation was practised.

The united testimony, however, of the old surgeons, who generally neither amputated nor adopted the method of resection, but who recommended and practised non-reduction, is, that it is much more safe to leave these bones unreduced, than to reduce them without resection; and I see no reason to doubt the correctness of their opinions in this matter. But whether it would be more safe to leave such limbs unreduced, or having practised resection to restore them, is another question, in which the advantage and comparative safety of the latter practice is too obvious to require explanation or defence.

Amputation.—"When this accident (dislocation of the ankle) is accompanied, as it sometimes is, with a wound of the integuments of the inner ankle, and that made by the protrusion of the bone, it not unfrequently ends in a fatal gangrene, unless prevented by timely amputation, though I have several times seen it do very well without."[1]

Says Sir Astley Cooper: "Thirty years ago it was the practice to amputate limbs for this accident (compound dislocations of the ankle-joint), and the operation was then thought absolutely necessary for the preservation of life, by some of our best surgeons." (*On Disloc.*, &c., p. 256.) Nor is it difficult to see by what reasoning they had fallen back upon this desperate remedy. Both reduction and non-reduction having proven eminently hazardous, in the absence of perhaps both knowledge and experience in resection, they finally adopted the alternative of amputation as that which after all must give to the patient the best chance for life; and were no other alternatives to be presented, this would be our choice in a large proportion of cases.

It must not be understood, however, that amputation is an expedient wholly free from danger; or indeed that the chances of the patient are in the average very greatly increased by this practice. Of thirteen amputations made for compound dislocations at the ankle-joint, in the Royal Infirmary at Edinburgh, only two resulted in the recovery of the patients.[2] Alluding to which, Mr. Fergusson remarks: "An amount of mortality which may well incline the surgeon to act upon the doctrine inculcated by Sir Astley Cooper." (To attempt to save the limb by reduction.) But Mr. Fergusson has added a sentiment which accords very closely with my own experience and opinions. "I fear, however, that in the attempts which have been made to save the

[1] Potts' Chirurg. Works, Philada. ed., 1819, vol. i. p. 248.

[2] Edinb. Med. Monthly, Aug. 1844.

foot (by reduction) the results in all the cases have not met with the same publicity; that the instances where amputation has been afterwards necessary, or where death has been the consequence, have not always been recorded; and from what I have myself seen, I would caution the inexperienced practitioner from being over-sanguine in anticipating a happy result in every example."—*Fergusson's Surg.*, p. 281.

By Tenotomy.—As a means of overcoming the resistance of the muscles, and for the purpose especially of facilitating the reduction, tenotomy has been proposed. (See *p.* 250 *of vol.* xxxiv. *of this Journ.*)

This method, based in some degree upon a very correct notion of the principal sources of difficulty and of danger in these cases, I regard as totally impracticable, at least to any useful or adequate extent. In order to be efficient all of the tendons passing the articulations must be cut, or nearly all of them; and I doubt whether the judgment of any discreet surgeon will ever sanction such an extreme, I might almost say, such an absurd measure. Nor do I think that in the point of view in which we are now considering this subject, having reference only to the question of danger, if the cutting of the tendons was sufficiently extensive to have any real effect in facilitating the reduction, the practice would be found to have any advantage over other methods known to be eminently dangerous.

By Resection.—Finally resection presents itself for our consideration as the only remaining surgical expedient.

We have seen that most of the early writers understood the effects of a constant strain upon the muscles in increasing the danger of spasms, inflammation and death; but in general they have suggested no remedy but non-reduction or amputation. Hippocrates, however, uses the following language, after speaking of resection of protruding bones in accidental amputations, or in fractures of the fingers: "Complete resections of bones at the joints, whether the foot, the hand, the leg, the ankle, the forearm, the wrist, for the most part, are not attended with danger, unless one be cut off at once by deliquium animi, or if continual fever supervene on the fourth day." (*Op. cit.*, vol. ii. p. 638.) To which passage the translator adds the following note: "This paragraph on resection of the bones in compound dislocations and fractures contains almost all the information on the subject which is to be found in the works of ancient medicine." Celsus notices the practice of resection in compound dislocations very briefly, as follows: "Si nudum os eminet, impedimentum semper futurum est; ideo quod excedit, abscindendum est."

Mr. Hey, of Leeds, was the first of the modern surgeons who called especial attention to the value of resection in compound dislocations.

Subsequently Mr. Parks, of Liverpool, in an "Account of a new method of treating Diseases of the Joints of the Knee and Elbow," advocates the practice of resection in certain cases of diseases of these joints, but especially in "affections of the joints produced by external violence."[1]

[1] System of Surgery, by Benj. Bell, Edinburgh ed., 1801, vol. vii. p. 360.

Mr. Levéille, in France also, following as he affirms the guidance of Hippocrates, has advocated a similar practice.

Velpeau,[1] Symes,[2] Fergusson,[3] Erichsen,[4] Miller,[5] Liston,[6] Chelius,[7] Lizars,[8] Gibson,[9] Norris,[10] under certain circumstances, and especially where the bones cannot otherwise be reduced, and where the dislocations occur in certain joints, and especially the elbow and ankle-joints, recommend resection. To which names we may add that of Sir Astley Cooper, who has considered the subject, as applied to the ankle-joint, quite at length, and who says: "I have known no case of death when the extremities of the bone" (tibia, at the ankle) "have been sawed off, although I shall have occasion to mention some cases which terminated fatally when this was not done."[11]

Why resection should diminish the danger to life, by placing at rest the injured muscles, has been already sufficiently considered; but it seems not improbable that, if synovial membranes are indeed more susceptible of violent and dangerous inflammations than the other tissues about the joints, then would this source of danger be removed just in proportion as the synovial membranes themselves are removed. Such indeed was the argument used by Sir Astley; and Mr. South, in a note to Chelius, when referring to this fact, has made the following statement:—

"In compound dislocations of the ankle-joint with protrusion of the shin-bone through the wound, most English surgeons saw off the joint end, not merely to render reduction more easy, but also, according to Sir Astley Cooper's opinions, to lessen the suppurative process, by diminishing the synovial surface. This mode of practice is certainly not commonly followed in reference to other joints, and the younger Cline was always opposed to it being resorted to in dislocated ankle." (*Op. cit.*, vol. ii. p. 251.)

Case of Compound Dislocation of the Tibia inwards, with Fracture of the Fibula. Resection of the lower end of the tibia, and recovery with a very useful limb.—Samuel Adamson, of Buffalo, æt. 24, was caught by the cable of a vessel June 17, 1855, dislocating the left tibia at its lower end inwards, and breaking the fibula two inches above the ankle. I was immediately called and found the tibia protruding through the skin about three inches. The periosteum was torn up, and the cartilaginous surface of the end of the bone was roughened. His thigh was also severely bruised and lacerated, but the bone was not broken.

Dr. Boardman assisting me, we attempted to reduce the bones, but with our hands we found it impossible to do so. I proceeded immediately to remove about one inch and a half of the lower end of the tibia with the saw.

[1] This Journal, vol. xxxiv. p. 250. [2] Principles of Surg., Amer. ed., pp. 332–3.
[3] Practical Surgery, 4th American ed., pp. 194, 202, 280, 281.
[4] Surgery, Amer. ed., pp. 239, 263. [5] Principles of Surgery, Amer. ed., p. 684.
[6] Practical Surgery, 1st Amer. ed., pp. 97–8.
[7] System of Surgery, Phila. ed., 1847, vol. ii. p. 251.
[8] Practical Surgery, Edinburgh ed., pp. 155, 160.
[9] Elements of Surgery, 2d ed., vol. i. p. 340. [10] This Journal, vol. xxxi. p. 15.
[11] Treatise on Disloc. and Frac., Amer. ed., 1851, p. 271.

The remaining portion was then brought easily into place, and the wound was dressed with sutures, adhesive straps, bandages, and light splints. On the same day he became an inmate of the marine wards at the Hospital of the Sisters of Charity, and was placed under the care of Dr. Wilcox, but through the politeness of Dr. Wilcox I was permitted to see him frequently.

The wound in the leg healed kindly, and with only a slight amount of inflammation and suppuration. Violent inflammation, however, occurred in the thigh, followed by extensive suppuration and sloughing. This, in fact, proved to be by far the most serious injury, and that which most endangered his life and delayed his recovery.

After about two months, the ankle was in such a condition as to require little or no further attention. The fragments of the fibula had shortened upon each other, and were united so that the tibia rested upon the astragalus. It was nearly two months, however, before he began to walk, owing to the condition of his thigh.

Aug. 24, 1856, fourteen months after the accident, Adamson called at my office. He is now employed again as a sailor on board the schooner Sebastopol, and performs all the duties of an ordinary deck hand. His leg is shortened one inch and a quarter; from which, it seems, that there has been some deposit upon the end of the bone, which has compensated for one-quarter of an inch of that which I removed. The ankle is perfect in its form, being neither turned to the right nor to the left, and he treads square and firm upon the sole of his foot. There is considerable freedom of motion, especially in flexion and extension. Occasionally it becomes a little swollen and painful.

The following case also, although not a dislocation, will serve to illustrate the same principle:—

Compound Comminuted Fracture of the Radius. Resection of a portion of the Ulna. Recovery.—William Croak, of Buffalo, æt. 30. Jan. 29, 1856, a large piece of iron casting fell upon his arm, crushing and lacerating the wrist, and comminuting the lower part of the radius; he was immediately taken to the Hospital of the Sisters of Charity. I found the whole of the soft parts torn away in front of the joint, and the fragments of the radius projected into the flesh in every direction. The hope of saving the hand seemed to be scarcely sufficient to warrant the attempt; at least by the ordinary mode of procedure. I, however, stated to the gentlemen present, among whom were Dr. Rochester, my colleague, and the house surgeon, Dr. Lemon, that I believed it could be saved if, having removed the fragments of the radius, we practised resection of the lower end of the ulna, and allowed the muscles to become completely relaxed. Accordingly, after placing my patient under the influence of chloroform, I enlarged the wounds so as to enable me to remove six or seven fragments of the radius, leaving others which were broken off but not much displaced. I then removed with the saw one inch and a half of the lower end of the ulna. The hand was immediately drawn up by the contraction of the remaining muscles, but their tension was completely relieved.

The wounds were closed and dressed lightly, and the whole limb was placed on a broad and well padded splint covered with oil cloth. The hand, which was very pale and exsanguine, was covered with warm cotton batting.

The subsequent treatment was changed from time to time to suit the indications; but his recovery was rapid and complete, nor was there at any time excessive inflammation in any part of the limb.

I have not seen him within the last two or three months, and I am at pre-

sent unable to say how useful his hand has become. I am satisfied, however, in a knowledge of the fact that it is saved.

In a case of compound dislocation of the upper end of the humerus, occurring also under my own observation, and recorded in the *Transactions of the New York State Medical Society for* 1855 (p. 27, Case 14), in which reduction was followed by death, I have now much reason to believe that if I had practised resection before the reduction, my patient's chances for recovery would have been greatly increased; perhaps, also, the case of compound dislocation at the wrist-joint recorded in the same vol. (p. 68), in which, having reduced the bones, I was subsequently compelled to amputate, may equally illustrate the hazard to which the practice of reduction without resection must often expose the patient.

The same remarks I will venture to apply to the case of compound dislocation of the hip, of which I have already spoken as having occurred in the practice of Dr. Walker, of Charlestown, Mass. Had the head of the femur been resected before its reduction, I cannot doubt but that the unfortunate man's chances for recovery would have been very greatly improved.

Thus, if we consider the question of the life of the patient only, the argument and the testimony seem to favour resection in a great majority of cases of compound dislocations occurring in large joints, and in a considerable number of cases of similar accidents in the smaller joints. It is certainly more safe than non-reduction or reduction without resection, and it is probably quite as safe as amputation or tenotomy.

But there is another question, which is, in our estimation, secondary to the one now considered, but which is often, in the estimation of the patient himself, of the first importance—namely, by which method will he suffer the least maiming or mutilation?

This question I do not find it difficult to answer. Certainly it is not by non-reduction or by amputation; and, putting tenotomy aside, it is now a question only between reduction without resection, and reduction with resection. These two methods, one of which experience has shown to be fraught with danger, and the other of which experience has shown to be relatively safe, are now to be compared in a point of view in which their antagonisms are perhaps less conspicuous, yet sufficiently marked.

First. In either case the inflammation consequent upon the injury may be violent, and the recovery slow and tedious. The same arguments, however, which we have applied to the question of the comparative danger of the two modes, must apply with nearly equal force to this question of maiming; since the amount of maiming must often be governed by the intensity and duration of the inflammation, and upon this point the testimony has been shown to be in favour of resection.

It will be observed that not only is the danger of maiming rendered more considerable by reduction without resection, because the inflammation is so

much more likely to extend to the tendons and muscles, causing them to adhere to each other, and to become subsequently atrophied, a condition from which they often never completely recover, but also because the ligaments and capsules of the joints, with the synovial surfaces, are in consequence encroached upon, and the freedom of motion is ever afterwards greatly restricted, if not completely lost. This marked impairment of the functions of the joint does not always happen, but it cannot be denied that it does generally. Indeed it is by no means uncommon for these accidents to be followed, after ulcerations of the cartilage, by copious bony deposits in and around the joints.

How is it, on the other hand, with these joints after resection? I have thus far heard of no cases in which complete anchylosis resulted; but in all considerable freedom of motion has returned, and in some the restoration in this respect has been nearly or quite as complete as before the accident.

Says Dr. Kerr, of Northampton:—

"Several cases of compound dislocation of the ankle have fallen under my care, and it has been uniformly my practice to take off the lower extremity of the tibia, and to lay the limb in a state of semiflexion upon splints; by this means a great deal of painful extension, and the consequent high degree of inflammation, are avoided. The splints I used are excavated wood, and much wider than those in common use, with thick movable pads stuffed with wool. I keep the parts constantly wetted with a solution of liquor ammoniæ acetatis, without removing the bandage. In my very early life, upwards of sixty years ago, I saw many attempts to reduce compound dislocations without removing any part of the tibia; but, to the best of my recollection, they all ended unfavourably, or, at least, in amputation. By the method which I have pursued, as above mentioned, I have generally succeeded in saving the foot, and in preserving a tolerable articulation."[1]

Sir Astley Cooper has made a valuable experiment to determine the condition of the new joint under these circumstances; and the vast number of cases in which resection has now been practised in cases of caries of the articulating surfaces, and their results, add still more substantial proofs as to the usefulness of the joints after such operations.

"I made an incision upon the lower extremity of the tibia, at the inner ankle of a dog, and cutting the inner portion of the ligament of the ankle-joint, I produced a compound dislocation of the bone inwards. I then sawed off the whole cartilaginous extremity of the tibia, returned the bone upon the astragalus, closed the integuments by suture, and bandaged the limb to preserve the bone in this situation. Considerable inflammation and suppuration followed; and in a week the bandage was removed. When the wound had been for several weeks perfectly healed, I dissected the limb. The ligament of the joint was still defective at the part at which it had been cut. From the sawn surface of the tibia there grew a ligamento-cartilaginous substance, which proceeded to the surface of the cartilage of the astragalus, to which it adhered. The cartilage of the astragalus appeared to be absorbed only in one small part; there was no cavity between the end of the tibia and the cartilaginous surface of the astragalus. A free motion existed between the tibia and astragalus which was permitted by the length and flexibility of the ligamentous substance above described, so as to give the advantage of a joint where no

[1] Cooper on Dis. and Frac., p. 275.

synovial articulation or cavity was to be found. This experiment not only shows the manner in which the parts are restored, but also the advantage of passive motion; for if the part be frequently moved, the intervening substance becomes entirely ligamentous; but if it be left perfectly at rest for a length of time, ossific action proceeds from the extremity of the tibia into the ligamentous substance, and thus produces an ossific anchylosis."

Second. Is it not probable, moreover, since the limb can be retained in place so much more easily after resection, that it will actually, in a majority of cases, be found to have been retained in place more perfectly? Even after simple dislocations, especially in those occurring at the ankle-joint, great deformity and much maiming are the not unfrequent results, and that too when all diligence and care have been employed. It has been impossible always to maintain a perfect apposition in the articulating surfaces. How much greater must be this difficulty in cases of compound dislocations!

Third. The only argument which remains in favour of reduction without resection is the necessary shortening of the limb after resection. But this need seldom perhaps to exceed three-quarters of an inch, and often not more than half an inch; an amount of shortening which, as I have had occasion to prove when treating of fractures, does not necessarily produce a halt, and which indeed is often not known to exist by the patient himself.

Finally, it must not be inferred that the writer intends to recommend resection as a universal practice in cases of compound dislocations of the long bones. He has only sought to determine in a general manner its relative value as compared with other modes of procedure; and especially has it been his intention to bring more prominently into view the importance of rest and relaxation to the muscles, as an element in the treatment most essential to success. To declare its special application to cases would demand a treatise more elaborate than it was proposed to write. If, however, one were to speak of the individual bones only, there seems sufficient authority in the facts and arguments already presented to conclude that resection is applicable to certain compound dislocations of the clavicle, humerus, radius and ulna, fingers, femur, tibia and fibula, and toes; in short, to all of these accidents occurring in the long bones of the extremities.

Art. III.—*Clinical Remarks on the Natural Range of the Heart's Sounds.*

By Chas. A. Lee, M.D.

Physicians who have not paid much attention to physical diagnosis, as connected with diseases of the heart, are often led astray by the signs derived from the extent of the sounds of the heart, without any regard to their

[1] Cooper on Disl. and Fract. p. 281.

quality. This mistake is rarely, if ever, made by those who have studied this class of diseases as a specialty, and who have had opportunity of verifying their diagnosis by autopsic examinations. I have frequently been called in consultation, where the patient has been supposed to labour under incurable hypertrophy, valvular, or some other disease of the heart, merely because the cardiac pulsations extended over a greater space than natural, and were distinctly heard in situations where, under ordinary circumstances, they are not heard at all. We do not refer to cases where the conducting power of the pulmonary tissue has been increased by tubercular deposit, pulmonary apoplexy, chronic consolidation of the lung or hepatization, the effusion of pleurisy, cancerous deposit, &c.; but where there is no evidence of pulmonary disease, or any mechanical cause which would be likely to increase or extend the cardiac sounds; and this error in diagnosis has been, no doubt, often occasioned by the erroneous teaching of some of our standard authorities on these affections.

Thus *Laennec*, the earliest writer on this subject, remarks that, "in a healthy person, moderately stout, and whose heart is well proportioned, the pulsations of this organ are only heard in the cardiac region, that is, the space comprised between the cartilages of the fourth and seventh ribs, and under the lower end of the sternum." (*On Diseases of the Chest*, Am. ed., p. 571.) This writer also states that in fat persons, the space in which the pulsations can be detected by the stethoscope, is sometimes not more than an inch square; but that, in thin persons, in the narrow-chested, and in children, the pulsations are more extended—being perceptible over the third, or even three-fourths of the lower part of the sternum, and sometimes over even the whole of this bone; also, on the left side, as high as the clavicle, and sometimes, though feebly, under the right clavicle.

Now, the fact is, as Andral has remarked, that it is not so uncommon for the pulsations of the heart to extend along the sternum and costal cartilages of the right side as far as under the clavicle, and that, too, in subjects not children, or thin, or narrow-chested; and so common is this the case in all possible conditions of health for pulsations to be heard in the right side of the chest, that it should not be considered as indicating any pathological condition. Yet Laennec tells us that "when the pulsation of the heart is heard over a greater extent than what is above stated to be the range of a well-proportioned organ, the individual rarely enjoys good health, and on examination will be found to labour under some of those symptoms of cachexia peculiar to some diseases of the heart; if he has not formal dyspnœa, his respiration is, at least, shorter than usual, and he is probably subject to palpitation." I need hardly observe that these statements are not supported by more recent observations. This writer, moreover, observes that when the pulsations of the heart become more extended than the points first mentioned, they are heard successively in the following places: 1. The whole left side of the chest, from the axilla to the stomach; 2. The right over the same

extent; 3. The posterior part of the left side of the chest; and 4. The posterior part of the right side; and the intensity of the sound, he remarks, is progressively less in the succession mentioned—that is, it is less under the right clavicle than under the left; less on the lateral parts of the left side than under the clavicle; still less perceptible on the right side laterally, and scarcely ever heard on the right side posteriorly. But there are so many exceptions to the above rule, as to render it of little practical value in diagnosis. For instance, in very thin persons the pulsations may generally be heard over the whole chest, both anteriorly and posteriorly; and in pregnancy it is very common to hear the first sound below the umbilicus. Bouillaud, in his work on *Diseases of the Heart*, tells us that he has, in the subjects belonging to the category specified by Laennec, not only heard the sounds of the heart once, but many hundred times in the regions indicated above; also, in all other parts of the chest, without excepting the right posterior region; also, at the lateral parts of the neck, a situation where, he states, they are very often almost as loud as in the præcordial region itself. The scale of Laennec, however, will not be likely to mislead, if we make ourselves fully acquainted with the causes and circumstances which produce an irregular propagation of sound, which will be noticed directly. This subject, of the extension of the sounds of the heart, derives its importance chiefly from the fact that Laennec, as well as other writers, regard such extension as a frequent sign of cardiac disease. Indeed, this writer observes that, "with regard to the relation between the state of the heart and the extent of its pulsations, it may be taken as a general fact, *that the extent of pulsation is in the direct ratio of the thinness and weakness of the heart, and consequently, inversely as its thickness and strength.*"

Laennec held that the size of the heart favoured the extent of pulsation, unless the walls were hypertrophied, when it was diminished. If the pulsations were heard over all the regions above designated, then we can safely infer, he says, that one or both ventricles are dilated, and this presumption will be strengthened if the pulsations are as great under the clavicle, or in the axilla, as in the region of the heart; but if the pulsations cannot be heard posteriorly, nor in the right side, while they may be heard in the other places, and if their intensity is nearly equal in all these, we may conclude that the ventricles are *moderately dilated*, and the walls of the heart naturally thin. This pathologist taught, also, that if the pulsations are felt very strong in the region of the heart, and are not perceived at all or only very slightly under the clavicle, we may conclude that there is hypertrophy of the ventricles. As a general rule, then, if the Father of Auscultation is to be believed, a great extent of sound is a mark of thin parietes of the heart, especially the ventricles; while a confined range of sound coincides with an increased thickness of these, and he considered this mode of graduating the extent of the cardiac sounds one of great certainty and practical value; indeed, he seems to have regarded his scale as one by which we might attain to nearly mathematical

exactness in judging as to the state of the cardiac walls. I need hardly say that the teachings of this distinguished pathologist, on these points, are regarded as of little positive value at the present time, especially when taken by themselves alone; in connection with other signs, they have a certain kind of value, as interpreting or corroborating them.

According to Hope, other things being equal, the sounds of the heart are louder, and of course audible over a greater extent of surface in *hypertrophy with dilatation;* while in simple hypertrophy, the impulse is increased, while the sounds are diminished. All writers on this subject admit that, in a mere thickening of the walls of the ventricles, without dilatation, the first sound is louder, shorter, and clearer than natural, somewhat like the second sound produced by a sudden extension of the semilunar valves. If the walls are dilated as well as thinned, then the first and second sounds become so similar that they can hardly be distinguished, except by position and their correspondence, or the contrary with the arterial pulse. As the flow of blood in the arterial tubes is retarded considerably in dilatation, the finger should be placed on the carotid, instead of the radial artery. We shall find the second as well as the first sound of the heart, increased in dilatation, unless there be softening present, when the sounds will be greatly modified.

In judging of the degree of dilatation, Dr. Hope recommends us to observe how far the first sound resembles the second, and to compare the intensity of the first, heard immediately over the ventricle affected, with what we conceive, from experience, would be its intensity in the same subject if the heart were healthy. This rule is, doubtless, superior to that of Laennec; and yet, it is not very easy of application in all cases. There are so many causes which modify the action of the heart, some of them altogether independent of its structure, and which, to a certain extent, moreover, are inappreciable, that any conclusions deduced from the application of this rule alone must be, to a great degree, unreliable. We should judge of attenuation by the first sound, as Hope states, less by its loudness than by its greater shortness and clearness; its more complete assimilation to the second sound. But I apprehend it would be very unsafe for us to diagnosticate dilatation with thinning of the ventricular walls, merely because we have a loud, quick, sharp, first sound, approximating to the second; for we often meet with the same sound, if I am not mistaken, in anemic subjects of a highly nervous temperament. Under these circumstances I have, in a fit of palpitation, often known the cardiac sounds audible to a considerable distance from the patient, while the whole chest was shaken by the violent throbbing of the heart. In a large majority of these cases of palpitation, there is undoubtedly, dilatation of the heart, with attenuation of its walls; but in a large number, the heart is found, on examination, to be in all respects normal. The opinion of Hope differs from that of Laennec in regard to the effect of dilatation with hypertrophy on the sounds of the heart; for while he believes that the effect is to render the sounds louder, the latter infers the contrary, and says we "may consider it as

constant, that the extent over which the beats of the heart are audible, *is in the direct ratio of the feebleness and thinness of its walls.*" Within certain limits, we should say that the opinion of Hope seems by far the most plausible; inasmuch as in very great dilatation, the walls will be unable to contract smartly, and the sounds must necessarily be weak, as is always the case near to dissolution and in ramollissement. But if hypertrophy exceed certain limits, then the sounds must be rendered comparatively dull from slow contraction. It is impossible, from the very nature of the subject, to lay down any rule of universal application.

There is no fact better known in cardiac pathology than that hypertrophy has the effect of deadening the sounds of the heart, especially the first sound, or that produced by ventricular contraction; and its dulness and prolongation will be proportioned to the degree of hypertrophy present; and I have known cases where it was nearly extinct. If heard at all it will be between the fifth and sixth ribs, or where the apex of the heart strikes against the walls of the chest, while the second sound will be heard best over the sigmoid valves and up along the course of the aorta and pulmonary artery. If there is contraction with hypertrophy, the sounds will be proportionably weaker. We need not remark that it is only in hypertrophy of both ventricles that we may expect the sounds confined within very narrow limits. In hypertrophy, then, with dilatation within certain limits, we have the loudest sounds, especially during palpitation, so loud that they may be heard over the whole chest anteriorly and posteriorly, especially in thin subjects and children. In proportion to the dilatation the first sound will be similar to the second, short and smart, from the sudden extension of the auricular valves; the second sound is louder than natural from the quickness of the ventricular diastole.

Mr. Hughes coincides in the views of Hope, viz: that there will be increased resonance of the sounds of the heart, when, the valves being healthy, the chambers of the heart are dilated and the parietes of the ventricles thinner than natural. He explains the phenomenon, also, in the usual way, that is, while the flapping action of the valves is free the contraction and dilatation of the ventricles are more rapid and smart; the agitation of the fluid which they contain is, consequently, greater than in a healthy state of the organ, and, of course, the vibrations communicated to the surrounding solids are increased. The *back-stroke*, so called, which is heard in these cases, is a loud, clear, and flapping sound, and is doubtless owing to the rapid dilatation of a large ventricle.

Dr. Gerhard states (*The Diagnosis, &c., of Disease of the Chest*, Phila., 1846) that simple loudness of the first sound of the heart depends either upon a temporary condition of the heart, that is, a simple febrile movement or nervous action, or upon a *hardening of the muscular structure* of the heart, perhaps, conjoined with slight obstruction of the semilunar valves, in which latter case "the increased loudness may continue for a very long period." But when the sounds are increased from this cause, there will be roughness,

or a bellows or rasping sound, some modification of the normal sound, and as to the increase of sound from a hardening of the muscular structure, as this would tend to impede the cardiac contractions, it may well be doubted whether the effect would be such as is suggested.

We have said enough to show that there is no natural and healthy limit to the heart's sounds even in health, and there is no practical utility in aiming at great precision in regard to this point. It is certain that the præcordial region does not define this boundary, for the second sound always exceeds that limit, being propagated along the aorta and pulmonary artery, and in regard to the first sound, there is an infinite variety in this respect; in no two individuals, indeed, can it be said that the sounds are limited to precisely the same boundaries. There are so many causes extraneous to the heart itself, which modify its sounds, that no precision can ever be arrived at in the rules laid down on this subject. In persons of a highly nervous temperament the sounds will be heard generally beyond the præcordial region even when not labouring under excitement; but if excited, or palpitation be present, there is scarcely any limit within which they are confined. They will, also, be of a higher pitch, and the extent to which they may be heard will be proportioned to the pitch. It is so in all cases of nervous palpitation, so called. Is there anything in the character of the sounds in such cases which throws any light on the question? Whether there be any dilatation of the heart or attenuation of its walls, we can only say that, as a general rule, clear and shrill sounds, if not particularly loud, indicate attenuation of the parietes, while both loud and clear sounds point to enlargement of the cavities as well as thinness of the walls; but a healthy or normal heart in a person of nervous temperament will, as before remarked, give forth loud sounds, which will be widely propagated over the chest. We saw this fact well illustrated in a recent case. We were summoned in haste to visit a lady who was said to be dying from disease of the heart. We found her homœopathic physician present, and the neighbouring women assembled "to be in at the death." The household were in dire commotion, the husband and children in tears and inconsolable. By examination no signs of organic disease of the heart could be detected except, perhaps, slight enlargement from dilatation, but there was no valvular disease. There was severe palpitation, which gave forth a sharp, clear sound, and this was propagated over the entire chest, anteriorly and posteriorly. It was hearing the sounds over the right side that led her physician to pronounce the case one of enormous enlargement of the heart, which must soon prove fatal. The fears of the patient having been allayed, and an opiate administered, the palpitations soon ceased, and the patient was, in a few days, in the enjoyment of her usual health. Such cases are often met with.

The extension of the sounds of the heart, then, does not necessarily indicate disease of the organ in a majority of cases; perhaps it points to some morbid condition of the lungs by which their power of propagating sound is increased. We have already alluded to some of these conditions, as tubercular deposit,

hepatization, pulmonary apoplexy, pleuritic effusion, cancerous infiltrations, pulmonary œdema, and, perhaps, dilatation of the bronchi, the existence of which can ordinarily be ascertained with great certainty by the history of the case and the usual diagnostic signs; while we are to remember that the sounds will be diminished, also, by a great variety of causes, as hypertrophy or thickening of the cardiac walls, causing slow but strong contractions, atrophy, softening, by anything which interferes with the free motion of the valves or the free action of the heart itself, local atony, general debility, anything which prevents full and free contraction, thus diminishing the sudden extension of the valves, as in hydro-pericarditis or emphysema of the anterior border of the left lung, want of pliancy in the valves, owing to ossification or atheromatous deposit, &c. Sometimes, also, the natural sounds of the heart are obscured by pericardial murmurs, or by bronchial rales; or, abnormal murmurs, arising from one valve, may hide or obscure the natural sounds arising from another.

I have intimated that our books attach too much importance to this increase of the natural range of the heart's sounds, as a sign of cardiac disease; whereas, it does not, necessarily, indicate any disease whatever of the organ, the cause, perhaps, being wholly extraneous. There is, also, much erroneous teaching in our standard works regarding this point. Thus, for example, the American editor of Williams' work *On Diseases of the Respiratory Organs*, remarks: "When the lungs are healthy, the intensity of the heart's sounds is directly as the distance of the point at which they are examined from their centre of production." No allowance is made for anything except the increased conducting power of the lungs from disease. The young practitioner needs being cautioned on this point, or he will be very likely to draw erroneous conclusions from this class of phenomena. In disease it is not the *natural*, but the *abnormal sounds* or *murmurs* he is to study, their character, their seat, their extent, and especially their causes. As a *point de départ*, he must accurately fix in his mind the natural situation and limits of the heart; its normal sounds, their causes and mechanism, their rhythm and usual boundaries, and the circumstances which lead to their extension; the relative situation of the different valves, their offices and functions, &c.; in short, everything relating to the anatomy and physiology of the organ. When this is all acquired he may then proceed to study its pathology, to learn how a modification of its sounds and motions grows out of change or modification of structure, as hypertrophy, dilatation, thinning of its walls, ossific or other valvular deposits, contraction of its cavities, softening, inflammation of its lining membrane or its peritoneal sac, polypus, malformations, aneurism of the aorta, &c. He will thus learn to distinguish structural from functional diseases of the organ, as *neuralgia of the heart*, or *angina pectoris, palpitation, syncope*, &c., and he will not be apt to confound simple extension of the normal sounds of the heart with organic disease of the organ; and this class of diseases, moreover, can only be studied to advantage in hospitals where numerous cases are presented, and where the symptoms during life may be connected with autopsic appearances. The ear

is to be cultivated by assiduous and systematic practice so as to detect the slightest deviation from the normal sounds, and this deviation must be at once traced up to its anatomical and physiological cause. There is something, moreover, peculiarly attractive and even fascinating to an investigating mind in the study of this class of diseases; there is not only the pleasure and the excitement consequent on the investigation of morbid phenomena, but there is the satisfaction growing out of a discovery of their causes, as confirmed by the unerring scalpel. In regard to many other diseases, where physical diagnosis is inapplicable and post-mortem research fails to reveal their secret and hidden seats, there is a painful uncertainty and mystery thrown around them, and so unsatisfactory have been all former attempts to reveal their pathological nature, that the mind instinctively shrinks, almost, from grappling with them. Such are tetanus, hydrophobia, neuralgia, &c. But, in diseases of the heart, the practised hand and the cultivated ear rarely meet with any insuperable obstacles. Here symptoms become *signs* which infallibly point to their pathological cause. Principles, and laws, and rules are thus deduced, which serve as a point of departure for all future study and observation.

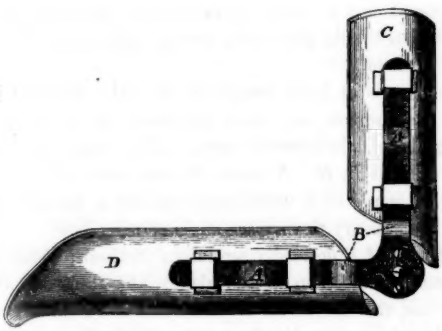
At present I have only taken up one phenomenon, one of the least important of all the phenomena connected with the heart, to show how, from imperfect observation, or rather from ignorance, its importance has been overrated, and how inferences have been drawn from it, not warranted by any of the facts of cardiac pathology, and that the existence of organic disease of this organ is to be ascertained by a different class of phenomena, viz., unnatural sounds or *murmurs, and by percussion.*

ART. IV.—*Description of a New Splint for Dressing Diseases and Injuries of the Elbow-Joint.* By HENRY BOND, M. D. (With a wood-cut.)

SEVERAL years ago, I had under my care a severe case of scrofulous disease of the elbow, where it was important to relieve the joint from the irritation occasioned by flexion, extension or pressure, and for a long time to keep remedies continuously applied to the part. The patient was a very responsible officer in a public institution of the first importance, and his services at that time were nearly indispensable, and required much time and very close and careful attention. It was, therefore, desirable to adopt a dressing that would interfere the least with the discharge of his official duties, and, at the same time, admit the most efficient treatment of the disease. I examined surgical books, and visited the shops of surgical instrument-makers; but I found nothing that seemed to meet satisfactorily the desiderata in his case. One objection was that none of them would afford a comfortable, as well as secure, rest or support to the affected limb; and this was a point of

some importance, at least to the patient, in a case where the dressing was to be worn continuously for months. As the joint, at first, was found at an inconvenient obtuse angle, and, if the limb were saved, it would probably be with anchylosis, one object was to bring it to a right angle, by the use of a jointed splint. But the chief objection was that, in cases of enlargement of the joint from whatever cause, whether from scrofula, as in the case then before me, or from the inflammation and tumefaction, very often attendant on severe injuries of the elbow, they must press upon the part affected, unless a large amount of cushions and compresses be employed; and whenever a splint requires this, it is an evidence of its imperfect adaptation to the case, and the difficulty of its application is correspondently increased. The unskilful use of compresses is the cause of a large proportion of the deformities resulting from fractures.

In this emergency I contrived a splint, which was made for me by my friend, the late Mr. John Rorer,[1] and which proved to be entirely satisfactory to myself and to my patient. As I see no one, among the several splints, illustrated in the recent report of Dr. F. H. Hamilton, nor in other late works on surgery, which seems to me so well adapted to the treatment of such affections, I will offer the contrivance to the trial of others, if you, Mr. Editor, shall think sufficiently favourably of it, and that I have not misjudged its utility. I do this the more confidently, from the statement of Dr. Mütter, that he had made use of it with entire satisfaction, and had exhibited it repeatedly to his class, pronouncing it superior to any other apparatus for the treatment of such affections.



Explanation of the Figure.—*A*, *A*, two steel plates, each about one inch wide and seven inches long, united in a joint, the motion of which is controlled by three screws. The central screw is the pivot upon which the joint

[1] This splint may be obtained of Mr. J. H. Gemrig, at 109 South Eighth Street, below Chestnut.

moves. The other screws are inserted through two fenestræ in the outer plate; and these fenestræ are of such a length or extent, as to allow the requisite variation of the angle, from a right angle to a very obtuse one, and so narrow that the heads of the screws when driven in will press upon their edges. Where the screws are partially withdrawn, the joint may be moved, and such an angle assumed as the limb to which it is to be applied may require; and then the screws are to be driven in firmly. Where the steel plates come in contact with each other, they should be made rough, or their polish taken off by filing or grinding; otherwise the screws may not always prevent the sliding of one plate upon the other. At *B*, *B*, about an inch and a half from the pivot-screw, there is a double bend in each plate, in order to throw out the joint so far (about ¼ of an inch), that it may not press upon or touch the swollen elbow.

C and *D*, two tin plates (tinned iron), each about four or five inches wide; the upper one (*C*), which is intended for the arm, about seven inches, and the other (*D*), intended for the forearm from nine to eleven inches in length, the dimensions of each varying according to those of the limb, to which the splint is to be applied. They may be made of such light tin-plate, as to be easily bent and adjusted to the limbs. The edge of the lower end of *D* is bent or turned outward, so that it may not present a sharp edge to the wrist or hand that may rest upon it. Upon the convex or outward side of each are tin loops for the insertion of the steel plates; and these are to be shoved in so as to bring the tin plates more or less near to the elbow, accordingly as its condition may admit or require. The tin loops are not attached midway between the lateral edges of the tin plates (as they appear to be in the wood-cut, which presents only a direct lateral view, or profile of the splint), but are so attached that the tin plates will extend quite round behind the arm, and under the forearm.

Where no artisan is at hand competent to make such a joint as is described above, the surgeon may direct any black or tin smith to rivet two steel plates together, at the required angle, and to make the double bend in each plate, as seen at *B*, *B*. A series of steel plates thus riveted together at various angles, would be a useful substitute for a movable joint in cases where it may be necessary to vary the angle of the elbow.

The patient was of a decidedly scrofulous diathesis, which had been repeatedly evinced by affections of the glands, the skin, and the eyes; and the treatment was, therefore, both constitutional and local. Tonics, both vegetable and mineral were employed; but the chief reliance was placed on the use of iron, in different forms, either the iodide or the tartrite in the form of a saline chalybeate, imitating the Cheltenham water. The following formula was employed much more than any other: ℞.—Magnes. sulph. ℥j; Potass. bitart. ℥j; Ferri sulph. gr. x. Dissolve this in a quart of water and take from ℥iij to ℥vj, daily, in divided doses, varying the quantity according to the condition of the bowels; using enough to keep them free, but not

enough to produce purgation. Bathing and attention to diet were not neglected. Sea-air and sea-bathing were resorted to for a short time.

Leeches and blisters were employed at first, and some use was made of tinct. of iodine; but the local remedy chiefly relied on was a solution of iodid. potass. (℥iv to ℥vj in water ℥xvj), sometimes alternated by the employment of a solution of muriate of soda. Strips of soft, thick muslin or canton-flannel, thoroughly wetted with the solution, were wrapped around so as to cover the affected part, and this was immediately covered with oiled silk. The silk protected the sleeve from the solution and prevented evaporation, so that by renewing the dressing every eight or twelve hours, the elbow remained constantly moistened by the solution; and this was continued many weeks. The splint, with a thin lining of flannel or wadding, was secured to the arm by a roller, or more frequently in warm weather by a few strips of muslin tied around.

This dressing, with the splint well adjusted to the size of the limb, added so little to the dimension of the limb, that the patient could wear a loose sleeve. A sling was sometimes used, but it was oftener omitted, the lightness of the dressing rendering it less necessary than is usual in the treatment of such affections.

The patient entirely recovered from the scrofulous affection, with some enlargement of the bones, and a partial use of the elbow-joint.

July, 1857.

ART. V.—*Successful Employment of Marshall Hall's Ready Method in the Case of an Infant born Asphyxiated.* By FREDERIC D. LENTE, M. D., of Cold Spring, N. Y. (With a wood-cut.)

SINCE the enunciation of Dr. Hall's *rules* for the recovery of drowned persons and stillborn infants, a sufficient number of instances of their practical application to such cases have been afforded the public by his medical friends in Great Britain to prove their great superiority over every method previously employed. I have not as yet met with any public testimony of their efficacy on this side the water, with one exception. A remarkable case of the recovery of a patient, poisoned by laudanum, by this method, was reported to the Society of Statistical Medicine, Jan. 12, 1857, by Dr. Lewis, of New York. [See *N. Y. Journ. of Med.* for Mar. 1857, and *Am. Journ. of Med. Sci.* for April.] Regarding this as one of the most important steps in the advancement of medical science, it affords me great pleasure to be able to present a very satisfactory instance of its successful application.

Mrs. H., aged about 25, was taken in her second labour June 29th, 1857. In the previous labour, the presentation was breech; and, owing to the delay

in the latter part of the labour from subsidence of the pains, the child was stillborn and could not be resuscitated. In this, her second confinement, the presentation was natural, and the labour progressed regularly and satisfactorily, the pains being very severe towards the last, until the head passed the outlet, when, as before, the pains ceased entirely. Finding, after a few minutes' delay, that the child was struggling for breath, and that pulsation in the cord had ceased, various means were resorted to with the view of exciting uterine contraction, but without the slightest effect, although the patient was not at all exhausted. Finally, ergot was administered in a full dose, and within a few minutes after this, and perhaps fifteen or twenty from the occurrence of the last pain, a powerful contraction expelled the remainder of the child. It was perfectly asphyxiated, there being no sign of respiration, and only one or two throbs of the heart at a long interval; the surface blanched; lips livid. A large vessel of warm water was in readiness, and into this the body was immediately plunged, and cold water immediately after dashed on the surface with a view to *excite* respiration; after a repetition of this without any success, a blanket was spread on the floor, the child laid upon it in the *prone* position, and what Dr. M. Hall terms his *ready method* of imitating respiration at once resorted to, the surface of the body having been previously dried. The body was first rotated gently on the *side* and a *little further*, describing an arc of a *little more* than 90°, by which means all pressure was removed from the walls of the thorax; as a consequence, a tendency to the formation of a vacuum was produced by their elasticity, and air rushed into the lungs. Turning the body back again to the prone position, the weight of the same, aided by gentle pressure along the chest posteriorly from above downwards, expelled the air from the lungs. At the same time, from the flaccidity of the neck of the child, it was requisite, every now and then, to give an extra turn to the head so as to bring the face *downwards*, and thus allow the tongue to fall forwards and drag with it the epiglottis, thus allowing a free ingress of air to the larynx, a point especially insisted on by M. Hall in his *rules*.

At first, no sign of a natural respiration was elicited, and the infant, with its blanched, cold surface completely exposed to the air, its flaccid limbs, and livid lips, certainly presented no very encouraging appearance to the bystanders, by whom such apparently trivial efforts at resuscitation were doubtless regarded with something akin to contempt; very soon, however, as the artificial respiratory movements went regularly on, the child gasped. At first, these gasps occurred at long intervals, that is, after about every three rotations, then after two; very soon, at each change of position, and then regular respiration, but exceedingly feeble, ensued. At the same time, the livid colour of the lips gave place to a roseate hue; a feeble attempt to open the eyes was noticed. This was not until artificial respiration had been kept up for perhaps twenty-five minutes; still the artificial respiratory movements were continued, and the natural respiration continued to improve regularly but very gradually, and then gentle frictions over the chest and extremities

with dry flannel had a perceptibly good effect, causing a frown and a feeble attempt at crying. At this juncture, I concluded to try the warm bath with a view to excite the feeble circulation, and this attempt demonstrated conclusively the wisdom of Marshall Hall's imperative rule—*not to excite the circulation of the blood before establishing the respiration*, for it was evident that the blood had not been sufficiently aerated, as was evinced by the respiratory movements becoming slower and feebler, and the colour of the lips changing again to a purplish hue; artificial respiration was immediately re-established with almost immediate effect; the child feebly opened its eyelids, and the respiration became more regular. After about three-quarters of an hour, or perhaps longer, from the commencement of the operation, the infant was wrapped in a warm blanket, care being taken to leave the face and upper part of the chest freely exposed, and not to overheat the surface—also to keep the child on its side, lest, in its extremely feeble condition, the tongue should fall backward, and interfere with the full establishment of respiration. Nothing more was done, except to watch the infant closely for the next two hours, and occasionally to use gentle friction over the body with flannel. It was not considered prudent to dress the child until after the lapse of six hours, at which time it cried strongly and appeared as strong as an ordinary infant. The child is now, ten days after birth, perfectly well and strong.

The above case is given with considerable detail for the reason that Dr. Hall's rules and observations,[1] so much commented on recently in the English

[[1] The accompanying wood-cut illustrates Dr. M. Hall's method:—

His directions are as follow:—

"Place the patient on his face, his arms under his head, that the tongue may fall *forward* and leave the entrance into the windpipe free, and that any fluids may flow out of the mouth; then

1. Turn the body gradually but completely on the *side, and a little more*, and then again on the face, alternately (to induce *in*spiration and *ex*piration).

2. When replaced, apply pressure along the back and ribs, and then remove it (to induce further *ex*piration and *in*spiration), and proceed as before.

3. Let these measures be repeated gently, deliberately, but efficiently and perseveringly, *sixteen times* in the minute *only*.—Editor.]

journals, have received but little of that attention in our journals which their novelty and importance demand.

Note.—Dr. Hall, at the suggestion of a friend, has called his the *ready method.* Would not the *physiological method* be a more scientific, and equally appropriate term, in contradistinction to the unscientific and empirical methods hitherto employed and still in vogue?

ART. VI.—*Extirpation of the Entire Clavicle.* By CHARLES R. S. CURTIS, M. D., Chief Surgeon to the North-western Eye Infirmary and Orthopædic Institution, Chicago.

ELIZABETH B——, aged 20, consulted me, about the 20th November, 1856, in regard to certain tumours existing on her neck and wrist. On examination, the tumour on her wrist was found to have a base about the size of a dollar, of an oval shape, and projecting about half an inch above the surface. It was situated on the anterior and internal side of the right wrist-joint, lying over the styloid process of the radius, the scaphoid, the trapezium, and the base of the metacarpal bone of the thumb. It was hard, almost cartilaginous, vascular, and exceedingly sensitive—the least touch causing acute pain. It was so firmly united to the deep tissues, that we could not determine whether the bones were involved.

This tumour first made its appearance when she was quite a child. She first remembers it as a small red excrescence, about the size of a pea. From this it gradually enlarged, until about four years ago, when it had attained the size of a hickory-nut. At this time it was excised by Dr. Cole, of Detroit. The operation, however, did not prove successful, as the tumour immediately began to reappear, and, at the end of a year, was again excised by the same surgeon; no portion of bone being removed during either of the operations. This operation, also, proved unsuccessful; and, about one year from the time of its performance, she had it *burnt* out with caustic of some kind, by a physician of the above named city, whose name I was unable to ascertain. This, again, was followed by a return and another removal by caustic at the expiration of another year. During all this time, the base of the tumour had been constantly growing larger, and about one year after the date of the last cauterization, she presented herself to me—the tumour appearing as I before described. The lymphatic glands in the course of the arm and axilla have at times been sore and enlarged.

About five years ago, she discovered a soreness at the middle of the subcutaneous surface of the clavicle, and soon after detected a slight enlargement at that point, which increased very slowly until within the last three months, during which time it has been growing much more rapidly, in fact enlarging nearly one-half. Pain has not been very great, but still there has been a constant soreness, slightly increased on pressure, *but not aggravated at night.* During the last three months, the pain and tenderness have been greatly increased. On examination, I found the tumour about the size of an egg, hard and unyielding, and exceedingly sensitive—the slightest pressure producing great pain. The crackling sensation, spoken of by authors, I was unable to

detect, though my friend, Dr. H. M. Kirke, thought he observed it. The soft parts above and below the clavicle were somewhat sensitive and painful under pressure, but not swollen. The lymphatic glands were not enlarged.

The patient was of full habit, sanguineo-lymphatic temperament, and has always enjoyed excellent health, *with the exception* of her having contracted syphilis about the time of the first appearance of the clavicular tumour. The syphilitic affection was entirely cured, and she has never had the slightest evidence of constitutional impregnation since. Her mother and one sister died of phthisis. No malignant disease is traceable in the family.

My first impression in regard to the clavicular tumour was, that it was simply an ordinary syphilitic node. But a careful examination of the history and general appearance of the case soon convinced me, as it did every physician who examined it, that we had a malignant disease to deal with throughout. An operation was accordingly advised. It was deemed expedient, in order the more effectually to secure her from a return of the disease, to remove both tumours at one time; and lest, by operating on the original tumour last, we might disturb the dressings of the clavicular wound, it was determined to dispose of that tumour first; and, accordingly on the 29th November, my friend, Dr. Kirke (the patient being thoroughly anæsthetized) proceeded to remove it.

The tumour was circumscribed by an incision through the healthy tissue, and removed with the fascia that participated in the disease. It was intimately connected with the deep fascia of the forearm and a small portion of the annular ligament; also with the sheaths of the tendons of the flexor carpi radialis and flexor longus pollicis. The radial artery was not divided. Hemorrhage was inconsiderable. The bones did not seem to be involved, and the wound was allowed to heal, by granulation, under the cerate dressing.

The patient being once more brought under the influence of chloroform, and placed on her back with the shoulders somewhat elevated, so as to depress the head and extend the platysma myoides, I made an incision about one inch above the clavicle, from the median line of the neck to a point a little beyond the anterior border of the trapezius, through the skin and superficial fascia. A second incision, parallel to this, was then made about one inch below the clavicle of the same length. These were united by a perpendicular incision in the form of the letter H. The flaps were now dissected back, exposing the lower portion of the platysma myoides. This muscle was carefully divided above and below from its clavicular attachments. It seemed to have lost its identity immediately over the tumour, apparently from chronic inflammatory action. The sterno-cleido mastoideus was now separated from its clavicular origin, by shaving the bone with the point of the scalpel. The trapezius, deltoid and pectoralis major were in a similar manner separated from the bone. It was found that the periosteum over the extremities of the bone was healthy, and, in order to favour the development of a useful callus, those portions of it were allowed to remain. The bone, being accurately dissected from its membranes at these points, an attempt was made to disarticulate it at its sternal end. This extremity being preferred in opposition to the method adopted by Drs. Mott and Warren, from the fact that after elevating it, in dissecting from the cardiac side of the vessels, there would be less danger of dividing them, and if divided, the ligature could be applied with greater facility. It was discovered, however, that the posterior portion of the orbicular ligament could not be incised without endangering important bloodvessels, and accordingly a different plan of procedure was adopted. About three-quarters of an inch from its sternal extremity, the bone was cut almost through

by means of a Hey's saw; the soft parts being carefully protected by the fingers of an assistant. A strong pair of bone forceps was now applied, and the division easily completed. A piece of tape was then passed beneath, and the bone elevated. I then proceeded to dissect the bone and tumour from their attachments. The soft parts were somewhat adherent, and exceedingly vascular over and around the tumour, and the blood flowed in a stream from every incision into it. Great care was taken, during the whole operation, to remove every portion of tissue that seemed to be in an unhealthy condition. As the posterior portion of the tumour lay immediately over the subclavian vessels, this dissection required, in its performance, a vast amount of caution. But, when we had passed beyond the tumour, the difficulties and dangers were greatly diminished; and the disarticulation of the acromial extremity was effected with comparative ease. But the sternal fragment was still remaining, and here the orbicular ligament presented some obstacles—owing to its close proximity to important bloodvessels, together with a slight peculiarity in that articulation—but by cautious dissection was finally divided, and the last portion of bone removed.

Not one of the large vessels was divided, although during the latter part of the operation the patient was restless and uneasy, from the fact that disagreeable symptoms compelled us to suspend the use of the anæsthetic. Hemorrhage was not very great, only two ligatures being required. The flaps were brought over and united by interrupted suture and adhesive straps. The arm and shoulder were supported by *Fox's Apparatus*, and the wound dressed with tepid water. The operation and dressing required nearly three hours. The patient was greatly exhausted, and fell asleep before the dressing was completed, not even waking when the stitches were passed.

During the whole time I was ably assisted by distinguished medical gentlemen of this city, and my obligations are especially due to Dr. H. M. Kirke, Dr. Wm. Narian, Dr. J. C. Thorp, and Dr. Banks.

The patient was placed in bed and ordered the following sedative diaphoretic mixture: ℞.—Liq. pot. cit., spts. mindereri, āā ℥ij; aq. camph. ℥j; morph. acetas gr. iv; antimon. et potass. tart. gr. j. S.—A tablespoonful every hour. But on account of the extreme sedation, it was discontinued and stimulants given. This sedation was evidently caused by the anæsthetic, of which ℥vj of chloroform was used. The stimulation was maintained until the evening of the second day—an assistant remaining by the patient's side until reaction was fully established. On the morning of the 3d December, the dressings were removed for the first time. Owing to the extreme mobility of the part, and the impossibility of keeping it at perfect rest, the wound, though looking healthy and suppurating finely, presented no points of union by the first intention. The stitches were removed, and the adhesive straps reapplied. The tepid water dressing was used throughout the convalescence. On the seventh day, she was sitting up, and the day following left her room, though she did not leave the house until the eighteenth day, when she presented herself at the office a mile and a half from her residence. She improved rapidly, and on the 30th December was discharged cured. The periosteum that had been left having thrown out considerable ossific matter; by the aid of a shoulder brace nearly all of the movements of the arm are accomplished, and the member is increasing in strength daily. The humour involved the outer portion of the sternal extremity and middle third of the clavicle, and presented every evidence of osteo-sarcoma. Unfortunately we had not the facilities for subjecting it to a microscopical examination.

June 7, 1857. Since the preceding was written, and about two months after cicatrization was completed, she began to complain of sharp, lancinating pains shooting up from the wrist to the shoulder. The cicatrices began to be sensitive with pain and tenderness on pressure. These symptoms gradually increased until the 14th of May last, when I find the following entry in my note-book: "Was called to see Lizzie B——; found her suffering great pain in wrist and neck, also along sternum; great tenderness on pressure over sternum, but no swelling. Auscultation reveals nothing unnatural. The wrist somewhat swollen, and has, on one or two occasions, bled profusely. The clavicular space continues to contract. Complains of choking sensations; pain in the head; constipation, &c. Ordered a mercurial.

"16*th*. Wrist and neck the same, other symptoms much improved."

It is now apparent, and indeed has been for some time, that a reproduction of the disease has taken place. I called on her to-day. The pain and tenderness have increased but little since the 14th of May. She is suffering again with torpidity of the bowels, pain in the head, nausea, &c., which were prescribed for as before.

Among the peculiarities of this case may be noted the rapid recovery from the operation, the slow and insidious course which the disease has pursued throughout, and its unfailing reproduction.

ART. VII.—*Smallpox and its Varieties.* By A. W. McDOWELL, M. D., of Bedminster, Somerset County, New Jersey.

THE question of vaccination, and its protective powers, has much agitated the profession. A large number contend that it ought frequently to be repeated. It is the duty of all of us to furnish such facts as may have fallen under our observation, and then to let the profession judge for themselves.

CASE I.—On the 20th of February, 1848, I was called to see James H., a young gentleman from New York, on a visit to his relations, who reside in the country. When I first saw him, he was complaining of a pain in his head, and had much nausea, which he thought was owing to a foul stomach, as riding in the cars had often affected him in such a manner. I prescribed an emetic of antimony and ipecacuanha, which relieved some of the symptoms, but he still complained much of his head. I left him a dose of calomel, to be taken in the morning, to be followed by Epsom salts. When this operated, his head seemed relieved; but as his stomach still continued much disordered, and there was much fever, I gave him an effervescing mixture and sweet spirits of nitre.

About the second day some vesicles began to appear upon his face and body. They came on gradually until there was a dozen upon his face. They were *conoidal*, with an inflamed border. On the second day they contained a pale yellowish fluid. About the fourth day some commenced scabbing, and left permanent marks. They *never* flattened. On the ninth day he was worse, and had secondary fever. At this time I had much chicken-pox in my practice, and pronounced this a case of the same kind; but I was much mistaken, as the sequel will show. It was a case of genuine varioloid.

Case II.—On the 17th of March I was called to see W., a young man in the same family where the above-mentioned young man was visiting. He also complained of his head, and called my attention to a curious eruption over his abdomen, which smarted and itched very much. It occupied the pubic, and extended over both iliac regions from one hip to the other; and presented exactly the appearance of a scarlet rash. There was no eruption elsewhere upon his body. There was no sickness of the stomach, and no pain in the bowels; the pain was confined to the head, and there was slight fever. I gave him an active dose of calomel and jalap; he was not confined to his bed. On the 20th, he was walking around out of the house; it was a raw, damp day. In the afternoon he sent for me; he was chilly, and felt stiff in his limbs. He still complained of his head; I gave him some antimony in solution—as much as his stomach would bear. On the 21st, continued the same treatment; on the night of that day he was quite flighty; on the 22d he was no better. I had hardly reached my home after the morning visit, when a messenger arrived and told me that my services were again required—that the young man was in fits. I repaired to the house immediately, and found him in convulsions, foaming at the mouth, and tossing his head incessantly from one side of the bed to the other; subsultus tendinum, so severe that it was only at intervals that I could judge accurately of the state of his pulse; constant motion of one arm or one leg; total inability to swallow; pupil of the eye dilated, and insensible to the strongest light; breathing stertorous; the rash had disappeared. In consultation with a neighbouring physician, we determined that our best chance was to bleed freely if the system would allow us. Accordingly we bolstered him up in the bed, and opened a vein. The blood flowed freely, but was very black. After bleeding we put his feet in warm water and ashes; he lay quiet while his feet were in the water. We then applied mustard sinapisms. We met again in the evening, and determined to bleed again, as the pulse had risen in volume. The blood was very dark at first, but gradually became of a lighter hue. After the first bleeding there were no more fits, but the other symptoms continued the same. We now commenced rubbing his spine with strong mercurial ointment. In the morning, the patient continuing the same, we gave him three drops of croton oil; this vomited and purged him freely; he passed his feces in the bed; his urine also flowed involuntarily. This evening, in swabbing his mouth, a little water trickled down, and he put up his hand and wiped it away; he also gaped. He was still insensible. On the morning of the 24th he awoke and spoke. He was perfectly unconscious of the interval from the morning of the 22d to the 24th; so that he lay insensible for forty-eight hours. The rash never returned upon his abdomen, but came out slightly upon his breast. His memory continued weak for some weeks, but he gradually improved and got perfectly well. He is a hearty man to the present time. It is a little singular, that a little girl, a child of this gentleman, had a similar eruption about the knee-joint, but had no fever with it. All these patients had been vaccinated.

Case III.—Working for the last-mentioned gentleman, who is an extensive farmer, were two black men, who with their families live in a house a short distance from the gentleman's dwelling. The one family consisted of the husband, wife, and four children; the other family, of a man and his wife. Of the children, two were vaccinated, two were not; of the other family the wife was unprotected.

The black woman with the family washed the last shirt worn by the first

patient. To this shirt some scabs must have adhered. After washing the shirt in warm soapsuds, she washed her children's clothes in the same water. The next Sunday she put clean clothes upon the child that was unprotected, and on the 21st this child broke out with genuine smallpox. On the 23d it began to show its true character. This black child had never been to the house; it caught the disease from the clothes. The other child, not having clothes on just from that wash, did not break out at that time. In this case the smallpox ran its ordinary course, and the patient got well.

Cases V. and VI.—In two weeks time from the commencement of the eruption in the last-mentioned patient, two others broke out with the smallpox—the other unprotected child and the man's wife; I vaccinated both of them, but without effect. They both had smallpox, and both recovered.

As they had but one room in the house for each family, the children that were vaccinated were in the same room with those that were sick, and were exposed to the contagion; they remained perfectly well. The mother of the children had been vaccinated several years before; she took care of the children; she had a slight rash upon her, was somewhat unwell, but did not get down. The husband of the black woman that had the smallpox was vaccinated twelve years before; he took the sole care of her, and was well all the time. On the day the first-mentioned child broke out, there was a black woman with her child visiting there; they were both unprotected. I vaccinated them; the ninth day of vaccination was the fourteenth day of exposure, when the second patient broke out with the smallpox—(I lost a few days by not having good vaccine virus.) But vaccination obtained the victory. She had a few pocks and some fever, but some Epsom salts soon cured her; the child remained well.

From these cases it can be seen that it is *not* necessary to vaccinate every seven years, as some contend; and that vaccination will check smallpox, even after exposure to the contagion, provided the virus has time to enter into the system. In connection with these cases let me mention an experiment I tried at this time. An elderly gentleman had been inoculated in his childhood with smallpox; I vaccinated him; it took well, and ran the usual course of vaccine virus.

Case VII.—At that time I had been vaccinated twenty-five years; it has never taken upon me since. I was exposed to all the contagion of smallpox. The varioloid and other patients did not affect me; but after being exposed to smallpox at the time the last-mentioned patient broke out, it disordered my head, giving me a very bad headache; it impaired my appetite so that I had no inclination for food, and my tongue was furred. I took a little physic, and in a day or two the symptoms passed off; at the end of two weeks these symptoms returned again, but in a much milder form.

These cases are interesting in several particulars. The varioloid in the first case so much resembled chicken-pox, that it was impossible to distinguish it by the mere eruption from some of my chicken-pox patients. The varioloid produced a singular rash in the second case. This rash never became vesicular or pustular—it was a simple rash. And yet this simple rash produced very alarming consequences after the patient exposed himself to cold. That varioloid, in the unprotected, produces *genuine* smallpox, thus showing the

identity between it and smallpox. And lastly, vaccination proved itself in these cases all it has been represented—namely, if fully in the system, it will completely preserve from the smallpox.

Case VIII.—On the 15th of December, 1848, I was called to see Mrs. F. The history of the case is this. She had been married then two weeks. On the day after her marriage her husband and herself went to New York on a pleasure trip. They put up at a hotel where a person, without their knowledge, had died of the confluent smallpox. Her husband had been vaccinated; she was unprotected. Two weeks after she had been exposed in New York she was attacked with very severe headache, pain in the back, sick stomach, and continued nausea; tongue much coated; breath very offensive. The day before she was taken sick she was present at a large party given in her honour. Her sisters spoke to her about her feverish breath. The next day she broke out with the smallpox. There were thirty persons present at the party, several of whom had not been vaccinated, and they were very anxious, dreading the disease; no one took it from her at that time. Hers was a very decided case of smallpox; under the usual treatment she recovered. Her husband, who had been vaccinated some twelve years, was with her during the disease; he was never sick. Her sister-in-law helped to take care of her (she was taken sick at her mother-in-law's house and could not be removed). She had been vaccinated fourteen years; she also complained of headache and pain in the back, with sick stomach. In the usual time the vesicles began to appear upon the body; but before they had half filled they shrivelled and dried up, and left no marks. Her mother-in-law had been vaccinated thirty years before; she had the same symptoms, and was quite sick for some days; some dozen pocks filled and scabbed. In her case the vaccination had become deadened, and she consequently suffered more. In these cases vaccination protected the husband, and much modified the disease in the other cases.

Just nine months from the date of her attack I attended this lady in her confinement, when she was safely delivered of a fine girl.

Case IX.—On the 29th of June, 1854, I was called to see a young lad in the village of P. The family, consisting of his father and mother, and several older children, moved from New York during the month of May. They occupied rooms in the upper part of the house. The older daughter had caught the varioloid in New York, and they brought the complaint along with them. They were all strangers to myself and the inhabitants. When I was called to see the lad they gave me no information, fearing the neighbours—knowing they dreaded the disease. When I saw the patient he was drawing his breath hard, and laboured very much; face flushed, pulse slow. There was evident congestion of the lungs; I thought that was the nature of the complaint. I gave him a dose of calomel, to be followed by castor oil, and put a mustard-plaster upon his breast. The next morning I called to see him; I then noticed a fine rash coming out upon him. I asked his mother, "Have you the smallpox?" "No, sir." "Have you the varioloid in the family?" She thought they had. "What had you done with the little boy before I saw him?" "I washed him with saleratus water." "Had he a rash?" She thought so. Then the disease was plain to me; it was smallpox. For three days after this he had constant spasms, with subsultus tendinum, and very flighty. The only way I could manage was to put him in a warm bath two or three times a day, and keep cold water to his head. I also gave him internal medicines; under this treatment the spasms subsided, and

the eruption came out upon him. I had to poultice his hands and feet to bring out the eruption upon them.

The weather was quite warm; the lad was in the second story, directly under the roof. When I turned the bedclothes down to look at him, a very strong odour arose. I thought if I escaped, vaccination was a sure protection. I did escape. I was vaccinated some thirty years before, and never since. His mother took care of him; she had been vaccinated several years before; she continued well. This was a malignant case, the weather was hot, the smell great, and we were all exposed; we depended upon vaccination alone, and that done many years ago, and that protected us.

These cases made me think that vaccination, *once properly performed*, is a sure preventive. All these patients got well; not one of the number died. These cases made a strong impression on my own mind, and I hope they will prove interesting and instructive to my younger brethren.

ART. VIII.—*Case of Successful Ovariotomy.* By HANFORD N. BENNETT, M. D., of Bridgeport, Conn.

MISS ALICE BASSETT, aged twenty-four years, consulted me early in the month of June of the present year, for an enlargement of the abdomen, which she attributed to a dropsical effusion. According to her statement, she had two years since a severe attack of measles, from which, however, she convalesced to a perfect restoration of health. In the fall of 1856 she first perceived, as she thought, some enlargement of the abdomen, which she then supposed was merely an increased corpulency, but at the same time felt, upon lying down, a peculiar sensation in the left side like the trickling of water along the abdominal walls. About the commencement of the present year, the development of the abdomen had become so great as to excite her alarm, but she consulted no physician, inasmuch as she felt no inconvenience from it, and perceived no depreciation of health with the exception of some emaciation. The abdomen continued to enlarge up to the period at which she consulted me, when it had attained the size and very much the form of the full term of pregnancy. She had menstruated regularly; the quantity of urine was neither much increased nor diminished; the alvine dejections were normal. Upon examination of the abdomen, fluctuation was very distinct, but it had this peculiarity, that when in the erect posture it was more perceptible at the superior than at the inferior part of the abdomen, and when lying upon the back there was no appreciable difference. Upon percussion the dulness was general, and no resonance could be heard in either iliac region. Although the diagnosis was obscure, I was strongly of opinion that the difficulty originated with an ovarian tumour, for more than one reason. The fluctuation was very distinct, and to my feel more superficial than usual in ovarian dropsy, that is to say, like that of ascites, but the obscurity of the fluctuation at the most dependent portion of the abdomen when the patient stood erect, led me at once to suspect that I had to do either with a multilocular cyst, the largest of the loculi being situated at the upper part of the tumour, thereby rendering fluctuation more distinct at that

point; or with a complication of ascites with an ovarian cyst, the latter rendering fluctuation indistinct in the erect posture at the inferior part of the abdomen by falling against the anterior abdominal wall, and thus interrupting the wave. Another substantial reason was the complete absence of any symptoms implicating any important organ of the body. The action and sounds of the heart were normal, and there was no œdema of the inferior extremities or of any other part; the fecal evacuations were perfectly normal in appearance, showing a want of even any *functional* disturbance of the liver; the urine was not albuminous, and, as I have before said, was neither materially increased or diminished in quantity. I at once gave my opinion to my patient, and informed her that the only prospect of cure was in an operation for the removal of the tumour, at the same time advising her of the hazards of the operation and its proportionate fatal issue, which I believed to be about one in three.

I did not see my patient again until after three weeks had elapsed, when I found her abdomen still more distended, fluctuation more distinct over the entire surface, and she was beginning to exhibit more signs of constitutional disturbance, general debility, loss of appetite, &c. I immediately determined to tap her as a means of confirming or invalidating my diagnosis. I introduced a trocar in the usual place, and when I had drawn off twelve pints of a tenacious fluid, thicker and more opaque than that usually found in ascites, the stream suddenly ceased, and I could not by any manipulation renew it, although the abdomen was not more than half reduced to its normal size. I withdrew my trocar, and could then distinctly feel a tumour in the lower portion of the abdomen, and occupying the median line. I now determined to operate as soon as my patient had recovered from the shock of the tapping, and without giving her time to become more exhausted by any increase of the effusion.

Upon the fourth day after tapping, being the third of July, I proceeded to the operation assisted by my friend Dr. W. L. Watson, of this city, and in the presence of several individuals, friends of the patient. I made an incision from the umbilicus to the pubis, carefully cutting through the peritoneum about midway between these points, as a first exploratory opening into the abdominal cavity. The moment the peritoneum was penetrated a gush of fluid took place, showing that the cavity of the peritoneum was the seat of a large effusion, but through the semi-transparent fluid I could discern the white cyst filling the lower part of the cavity, and presenting as it lay *in situ* a somewhat flattened superior convexity. I enlarged the opening in the peritoneum to the full extent of the incision of the integuments, and after allowing all the fluid to escape, I grasped the tumour and carefully drew it through the opening. I first experienced some difficulty in accomplishing this, and began to fear there might be adhesions to the sides of the pelvis, but upon making still stronger traction the tumour slipped through the incision without further difficulty, the resistance having been due, as I suppose, to the close impaction of the tumour in the pelvic cavity, and the tenacity of the thick glutinous fluid which covered its surface. I then terminated the operation in the usual way by passing a needle armed with a double ligature through the pedicle, tying both parts firmly, and cutting away the tumour. Having carefully sponged the wound and closed it with four sutures, not including the muscles within them, I applied adhesive straps to the surface, and pinned a bandage around the abdomen in the same manner as in childbed. During the operation, the patient was kept steadily though moderately under the influence of chloroform by Dr. Watson.

I should observe that the day previous to the operation, I directed my patient to take a mild cathartic, which evacuated the intestines freely. After the operation, as soon as she had become fully roused from the influence of the chloroform, I administered a full dose of morphia, which had the happy effect of lulling all pain, and during the afternoon she slept soundly for two hours. It is sufficient to say here, that my patient has had an uninterrupted convalescence, no untoward symptom arising to cause alarm or call for interference. I kept her bowels quiet with morphia, and drew off her urine until the eighth day, at which time I administered a full dose of Epsom salts, and during their operation she passed her urine voluntarily, and has continued to do so from that time. The ligatures came away on the fourteenth day, and the cure was complete, the wound being perfectly consolidated. Just five weeks after the operation she walked a distance of two miles without more than ordinary fatigue, she has again menstruated normally, has a strong appetite, regular alvine dejections, and says she was never in better health.

The tumour was a multilocular cyst, assuming very nearly a globular form after extirpation, and measured seventeen inches in circumference. The term *multilocular cyst* commonly used to designate these morbid growths, does not seem to me to describe their anatomical character, because they are evidently not cysts with several compartments, but a congeries of cysts, one growing within the other, and thus multiplying in some instances to an infinite number, and varying in size from one capable of containing a gallon or more of fluid, to those of only microscopic appreciation. The largest in this tumour had not a capacity exceeding six fluidounces.

Remarks.—Notwithstanding the frequency of the operation of ovariotomy in this country, and the success which has attended it in the hands of some surgeons, many eminent members of the profession, and some of our first operators, are disposed to doubt its propriety. I believe this is due in part to the too great fear of wounding the peritoneum, and the idea of its excessively inflammable character; and in part to the failure of operations in cases badly selected. I shall remark nothing further in reference to the first point, except that so far as I can gather from my own experience and that of others who are familiar with the subject, the dread of inflammation of the peritoneum after operations involving its section, is unnecessarily great, and that our ideas of the highly inflammable character of serous membranes are derived from idiopathic inflammations which are generally of a diffuse character, and consequently attended with great fatality, while experience shows that inflammations the result of traumatic lesions have a strong tendency to become circumscribed and to terminate favourably.

I believe the judicious selection of cases to be the chief point to be considered in reference to this operation, and first as to a diagnosis of the tumour. It is a common opinion, and one derived from the books, that ovarian tumours are usually situated upon the side to which the diseased ovary belongs. Cruveilhier declares this view to be purely theoretical, and maintains that these tumours take the median line as soon as they attain any size, for the reason that it is in this direction they are most easily developed.

This is undoubtedly true of those cases in which the tumour is rapidly evolved, and where its contents are of a fluid and yielding nature; tumours of this character take their place almost immediately upon the median line, and are generally unaccompanied by pain; but when they are of slow growth, of a solid character, and attended with considerable pain during the first period of their development, they then contract adhesions to the sides of the pelvis and to the pelvic viscera, and may for a time retain a lateral position in the abdomen. The history of the case should be closely investigated in reference to these points as under the latter circumstances the surgeon should in my opinion hesitate as to an operation. If the tumour has been of rapid growth, has early taken a position upon the median line, and has been unaccompanied by much pain, I consider the success of an operation as highly probable, with favourable conditions of the general health of the subject; but when the morbid growth has been slow, probably of a sarcomatous nature, inclines to a lateral position in the abdomen, and has been accompanied by severe pains in the pelvic region and through the hip and back, I believe the operation generally unwarrantable, inasmuch as there will probably be encountered adhesions so extensive as to render it difficult, and the breaking up of which, if possible, would insure a deadly peritonitis.

Another important point to be considered is the age of the patient. I should always prefer to operate upon a young subject, at least not one above forty. Ovarian tumours are developed much more rapidly, and are also more rapidly fatal in younger females, especially where the contents of the tumour are of a fluid character. A few months is time enough to develop a sac sufficiently large to fill the cavity of the abdomen, and if tapping is resorted to, the patient succumbs in a short time to the drain upon her system. Hence I believe it to be highly important that the operation should be performed before tapping, or if, as in the present instance, it is thought best to tap in order to arrive at greater certainty as to the nature of the case, to operate before the patient has become further exhausted by an increase of the effusion. The fact that young subjects die much sooner of this disease than those more advanced, is to my mind a strong inducement to the operation in such persons.

In conclusion, I consider the suggestions of my uncle, Dr. E. P. Bennett, in reference to this operation, as highly judicious and worthy of repetition, viz: If the patient menstruates, select the period for operating immediately after the cessation of the menstrual flux; evacuate the bowels freely by a cathartic the day before the operation; draw off the urine for several days after, and keep the bowels quiet with opiates (in the present case I continued this to the eighth day); keep the room as nearly as possible of an even temperature; and also subdue pain and procure sleep with sufficient anodynes.

Bridgeport, Conn., Aug. 10, 1857.

ART. IX.—*Two Cases of Recovery from Fracture of the Spine, with Remarks on this Fracture.* By FREDERIC D. LENTE, M. D., Surgeon to the West Point Foundry.

FRACTURE of the spine, excluding the case of partial fracture, as where the spinous processes merely are knocked off, is commonly regarded as necessarily fatal, and, judging from the published opinions of the great authorities, recovery from complete fracture is one of the rarest events in surgical practice. Thus, Erichsen, in his *Science and Art of Surgery*, says, "it is inevitably fatal." Benj. Bell, in his *System of Surgery*, says: "Examples of recovery are recorded, but they are extremely rare, and to be ascribed to the secret operations of nature rather than to the efforts of art." Sir Charles Bell, in his lectures on the *Spine and Thigh Bone*, says: "Although many have survived such fractures for a considerable time, yet they generally linger and die of the consequences."

Sir Astley Cooper appears not to have met with an instance of recovery from this injury. He relates two cases where patients survived a long time, one two years, the other nine months, but finally died of exhaustion, from sloughing, &c. Such being the case, it is hoped that no apology will be deemed necessary for introducing the two following cases. We give them at length, at the risk of being considered tedious, not only on account of their rarity, but because the favourable result seemed, in some measure at least, due to the treatment employed, as well as to what Benjamin Bell calls the "secret operations of nature."

CASE I.—Barny McGuire, aged about forty, of ordinary health, a "helper" in the foundry, fell a distance of twelve or fifteen feet while at work on the 9th July, 1853, striking on his back. I did not see him until about four hours after the accident, when I found him in the following condition—he was lying on his back in bed, his countenance pale and indicating great anxiety and prostration, intellect perfect; pulse rather feeble; stimulants had been already administered, was complaining of severe pain in the dorsal region, and "numbness" in the lower extremities. Had not passed his water since the accident. There was complete paralysis of motion in the left limb, and almost complete in the right, being just able, with great difficulty, to flex the knee slightly, and to "stir the toes."

Upon examining the back, the only point at which he complained of pain, I found a considerable projection at the situation of the lower dorsal vertebræ; upon pressing upon which, patient complained of great pain and soreness, so that it was difficult to make out whether it was due to extravasation or to injury of the bones. He was immediately cupped over this point, and directed to be kept as quiet as possible in bed. After the cupping, a little power of motion in the right leg was gained, and he complained of a pricking sensation in both limbs. Paralysis of sensation was evidently less. His urine was also drawn off.

July 27. The improvement of the patient, up to this date, has been very

slight. Sensibility has been, in a great measure, regained in both limbs, but he still complains of pains darting down them, and of the disagreeable pricking which followed the cupping. It is impossible to maintain a proper temperature in the feet and legs even by frictions and hot applications. He can now flex the right thigh tolerably well, and the leg on the thigh, but cannot raise the limb bodily from the bed, nor can he flex the foot, nor flex nor extend the toes. In the left limb but little improvement has taken place, still there has been some. His water has been drawn off regularly three or four times daily, and lately, it has been slightly alkaline, of an ammoniacal odour, and a deep brown colour. If allowed to remain in the bladder longer than six hours, patient complains terribly. He is still unable to change his position in bed without assistance. He sometimes lies on his back, sometimes on his side. The extravasation has now disappeared, and all doubts as to fracture have been removed. There is a marked projection of the spinous processes of the tenth and eleventh dorsal vertebræ, and pressure at this point still gives pain; when patient is moved in bed he fancies he feels the bones move upon each other; this sensation, he invariably feels when his position is changed. Bowels torpid, and moved regularly by enemata and cathartics. Muscles of lower extremities considerably attenuated. Has been taking infus. fol. buchu and acid. sulph. aromat. for the vesical difficulty.

July 29. Commenced yesterday the application of electricity by means of the rotary magneto-electric apparatus; after two applications, patient was enabled, with considerable pain and difficulty, to pass his water. To take a pill composed of strychnia gr. one-twelfth, ferri sulph. gr. j, aloes Barbad. gr. j, three times a day.

Aug. 12. Have not had occasion to use the catheter since the electricity was commenced, though sometimes patient experiences great difficulty in passing his water; its colour and odour have improved under the acid and buchu, and it is less alkaline. The sensibility of the extremities has been almost completely restored, and the pains and pricking sensations have almost completely disappeared. The motive power is steadily improving, but very gradually indeed. He can, with the assistance of his hands, cross the right leg over the left, and flexion of the thigh and leg is performed with considerable facility, but he is still unable to raise the limb bodily from the bed. The improvement of the left has not kept pace with the right, but it is, nevertheless, very apparent. The strychnia has a very obvious effect, producing almost constant tonic spasm of the muscles, and stiffness of the knee and ankle-joints. When intermitted for a time, the spasm soon subsides. The bowels still inclined to be torpid.

Sept. 25. Patient has been slowly but steadily improving since last date. The same treatment has been continued. The electricity was applied every day for two weeks; then every second day, and now every three days. The pills were intermitted for some days, but patient thought he did not improve as fast without them, and they were recommenced. He takes now one-eighth of a grain of the strychnia twice a day, and the tonic spasm is still produced by it to some extent. If he takes three a day, the stiffness of the joints is such that he cannot move his limbs to any extent. Had incontinence of urine for a few days, which was relieved by tinct. ferri muriat. and tinct. canth. His condition is now as follows: The prominence of the spine at the seat of injury remains the same, and forms quite a salient angle. He can stand alone with some difficulty, if supported by a cane, and can hobble

along the floor of his room slowly with the aid of a cane and crutch. Ordered to take one pill a day, and to discontinue electricity.

March 14, 1854, eight months from the date of the accident. Patient has been walking about the street for two or three months. His gait improves very slowly. He can walk about two hundred yards without requiring to rest. Has not been under any special medical treatment for the last four months.

May, 1857, nearly four years from the date of the accident. Patient has been getting regular wages in the foundry for more than two years. His work requires him to stand on his feet part of the time and to sit part of the time. The muscular power of the *right* leg is much better than that of the left. He can stand alone on this leg, but cannot on the other. When he mounts a flight of stairs, he always raises the right foot, and drags the other after him; sometimes he mounts two steps at a time. On the other hand, the *sensibility* of the *left* leg is much greater than that of the right, but the sensation of both is considerably impaired. He can walk for a quarter or half a mile without resting. He cannot stand at his work without support more than fifteen or twenty minutes at a time. Occasionally he requires to pass his water very frequently through the day, but never involuntarily, and does not often require to rise at night. His general health now is better than it has been for years. No pain or pressure now at the seat of injury. The prominence remains the same. His gait in walking is a very peculiar one, seemingly requiring great exertion; he seems to throw the limbs bodily forwards from the hip-joint.

Case II.—This patient, a strong, healthy young man, aged about 20, an apprentice to a tinman, was stepping from the roof of a house to a narrow scaffolding below, when he lost his balance; finding that he was about to pitch head foremost to the ground, a distance of about thirty feet, he sprang forward, and alighted first on his feet, and then received the force of the concussion on his seat. The ground was firmly packed clay and gravel, yet the force was so great that his buttock made a distinct depression. He was taken up pale, exceedingly prostrated, and almost senseless; he soon, however, recovered to some extent. It was found immediately that he was completely helpless below the hips, and he was taken home on a cart.

Upon a thorough examination of the patient, after having him placed on a bed, and after reaction had set in, there was found to be complete paralysis of *motion* of both lower extremities. Paralysis of *sensation* was complete, on the *right* side, from a little below the crest of the ilium downwards; on the *left* side, the paralysis of sensation was not so complete. Paralysis of the *bladder* complete. Upon pressing along the spine, no pain was complained of until the lumbar region was reached; at about the middle of this, the tenderness was very great, but there was no perceptible irregularity. The patient remained under my care for about two weeks, and was then removed on a litter to Orange County, where his parents reside; for the further history of the case, and its treatment, I am indebted to the kindness of his physician, Dr. J. H. Thompson, of Goshen, whose own language I shall preserve, as far as possible, in continuing the case.

"When he first came under my care, in October, 1853" (that of Dr. Thompson), "he was entirely helpless; he had not the slightest power of *motion* in either limb for at least three months after the injury." Paralysis of *sensation* was complete on the *right* side below the hip-joint; on the *left* side, sensation was much impaired, but not obliterated. The catheter was

required for *five months* regularly; there was a good deal of pain complained of at the seat of injury; indeed, all his suffering was referred to this point; upon the slightest motion being given to his body, or on the slightest pressure, this suffering was much increased, he had not power to move himself in any degree.

"At first, it may be remarked, the question to be decided, with respect to the case, was as to whether there was total destruction of the spinal cord, or some injury less serious. I must acknowledge to you, from what I know of the history of the case, the nature of the accident, and the condition of the patient, I apprehended, in the beginning, that the employment of remedies with a view of even partial cure of the paralysis would be discouraging indeed. The very slight motion, however, at the end of several months from the receipt of the injury, perceptible though slight, stimulated my hitherto feeble hope in reference to improvement, and encouraged a perseverance in appropriate remedies to that end."

"With regard to treatment, I used externally embrocations of various kinds to the seat of injury, and once, a blister was applied. Subsequently, to relieve the spinal irritation, which undoubtedly existed at first, a seton was introduced; also a strychnia plaster combining a counter-irritant." The internal medication consisted in "strychnia, in doses of one-twelfth of a grain from the first, which was continued at least one year. He began with two pills a day of the dose I have mentioned, gradually increasing to three or four a day, according to the effect. Indeed, at one time, he hazarded as many as six and even seven a day. This he did on his own responsibility, and I need hardly state, with alarming consequences; however, he subsequently took five of these pills a day with advantage. After he had taken the strychnia about a year, it was alternated with pulv. nux vomic. in doses of one-fortieth of a grain with good effect, taking two pills of the strych. and two of the powders daily. After continuing the remedy for a year and a half, it seemed to fail to produce any marked effect. *Strychnia* and *nux vomica* were the sheet anchor in the treatment, and its success exceeded my most sanguine expectations. Electricity was also employed, but, I fancy, without benefit." With regard to the progress of the improvement of muscular power: "In the beginning, that is between three and four months after the accident, it was hardly perceptible, and improved gradually, and very slowly of course. It was first observed in the *right* foot, that in which the paralysis of *sensation* was complete."[1]

"Patient had a very bad bed-sore in the early part of his illness, occasioning some sloughing of the soft parts, and necrosis of the bone. This difficulty caused considerable trouble; exfoliation occurred, and finally the wound filled with healthy granulations, and cicatrized; it was apparently the last lumbar vertebra."

The following is the patient's own account of his case in a letter to me, dated Feb. 26, 1857: "I was confined to the bed seven months; then I commenced sitting up a little every day; I commenced standing on my feet (with support), and walking around the room with two chairs in eight months. It was eighteen months before I used the crutches, or rather the contrivances I had when you saw me. I could use my *right* limb so as to bend it in the knee in four months. I could pass my water without assistance in five

[1] I have had to draw up the case from letters written at various times by Dr. Thompson, in answer to questions from me, which will explain its somewhat disconnected character.

months. I have no trouble about it now. I have never any painful sensations, except in my *right* limb, the one I have the most use of. The sensation consists in a quick, sharp, pain, principally in the knee, which comes in and goes out very quickly; I can see it draw my foot up in the ankle, but I *cannot feel it there.* In that limb (the right), I cannot feel it if I stick a pin anywhere below the hip-joint. In the other limb (left), I have nearly the natural feeling; have never had any pain in that limb. I can bend the limbs in the knee, thigh, and hip-joints. I can bend the *right* nearly as well as any person. I cannot quite stand alone. I do not think that I have gained any in the last year; have used no remedies the past year."

When I last saw my patient, as he states, all improvement had ceased for a year, and probably forever. He was then driving about the country in a wagon by himself, selling books, prints, &c., and walking about when occasion required. His contrivance, referred to in the above letter, for assisting locomotion, is ingenious, and much more useful, in such a case, than ordinary crutches. It consists of two stout walking canes, terminated above by a cross-piece for the hand, and below by a broad thick shoe or piece of plank about 6 by 12. He first puts one forward, then swings forward the leg, then places the other in advance, and then swings forward the other leg, and so on. He flexes his knees but slightly in walking, and throws out his legs with a swinging motion from the hip-joint. Patient's virility is not impaired; general health as good as ever. Dr. Thompson lays considerable stress on the patient's "unprecedented hopefulness and courage," under the most discouraging circumstances, as conducing, in no small degree, to the favourable result which ensued.

There are several notable features in the above cases, upon which it may not be out of place briefly to remark. The fact that, in both cases, the muscular power was greatest in the limb having the least sensibility, in one no sensibility at all; the long continued use of *nux vomica* and its alkaloid, and its undoubted efficacy in both cases, is a feature also worth noting. I believe so decided evidence of the good effect of this agent in paralysis from injury has seldom been observed or recorded. The most important point, however, is the recovery of two cases of complete fracture of the spine, whether this result be attributed to the treatment at all, or to the powers of nature, in resisting and repairing the effect of severe injury, or to both. This is a point of considerable practical importance. Complete fracture of the spinal column has almost universally been regarded by authorities, as will be seen by the references at the beginning of this article, as necessarily fatal. In consequence of this, no special efforts are usually made with a view of curing or partially curing the case, or of prolonging life until the recuperative power of the system may step in, and assist us in alleviating at least the wretched condition of the patient. How do patients usually die with this injury? It is generally by exhaustion of the vital power caused, in a great measure, by the constitutional irritation and debility consequent upon the terrible bed-sores and the subacute inflammation of the mucous coat of the bladder, due, probably, to the irritating effect of decomposed urine, and even of healthy urine on the surface of an organ deprived of its wonted nervous

supply. The only chance then for the patient consists in warding off, as far as possible, these contingencies by the most assiduous attention. In many cases, no doubt in a vast majority of cases, we shall fail to do so, even with the aid of water beds, and all the other appliances which modern ingenuity and humanity have devised; but these two cases, especially the last, where long continued paralysis of the bladder, and a bed-sore, involving caries and exfoliation of bone, did not preclude a partial recovery, and without any special or expensive appliances for promoting the comfort of the patient, prove that, occasionally at least, we may succeed, and should encourage a steady perseverance in all the minutiæ of treatment which may tend, in the slightest degree, to facilitate so desirable a result. A frequent catheterism and a scrupulous attention to the cleanliness of the patient, and protection from pressure, of those parts of the body which come in contact with the bed, form the principal prophylactics; while strychnine, both endermically and internally, and balsam of copaiba or buchu, where inflammation of the bladder is threatened, should be perseveringly employed.

The manner in which fracture of the spine usually occurs, or rather the mechanism of the fracture, if I may so express it, should also encourage a more hopeful prognosis in these cases. In most cases, according to the dissections which I have made and seen in fatal accidents of this kind, the fractured vertebra is crushed between those adjacent to it, and sometimes diminished to almost one third its natural thickness in front; just as if the force had been applied in a directly vertical manner, as it was in Jones' case, but as it is not generally. Sometimes, as in a specimen in my collection, the fragments of the body of the bone are forced backward, so as almost completely to close up the vertebral canal, but it may not be diminished but little, as in another specimen in my possession; in an occasional case, perhaps not at all, as the fragments happen to be forced in a different direction, or as the injury is less extensive. This, no doubt, happened in McGuire's case, although the crushing of the vertebra must have been very great from the amount of deformity still existing.

Note.—Just after concluding this article, I chanced to meet Mr. Jones, and have learned some additional particulars of interest concerning his case. His condition is just the same as it was when he last communicated with me by letter in February. He has no power whatever in the flexors or extensors of the feet, or in any of the muscles of the feet, consequently, they hang helpless and useless. The muscles of the thighs and legs are very much attenuated, not more so, however, than they were a year ago. He states that he never walks over eight or ten rods at a time; if he attempts to do more, he gets very much fatigued; he is able, however, to get into his wagon which is low, and to go about from place to place alone. He also states that, for some months after the accident, there was great rigidity of the knee and hip-joints, and it was with great difficulty, and considerable pain that they could be flexed, but forced flexion and passive motion were kept up by himself and

friends, together with frictions, with great perseverance for about a year before the rigidity was entirely overcome. He thinks that it was only by these constant efforts on the part of himself and friends for so long a time, that he was enabled to overcome the difficulty, and to gain what muscular power he now possesses, aided, of course, by the medical treatment.

Cold Spring, N. Y., *Aug.* 14, 1857.

Art. X.—*Poisoning by Chloroform.* By Charles H. Smith, M. D., Assistant Surgeon, U. S. A. (Communicated by Thos. Lawson, M. D., Surgeon-General, U. S. A.)

Private Henry Thompson, Company F., Second Cavalry, having a few days previously made an ineffectual attempt to destroy himself, which resulted in a slight wound; on April 13, to carry out his design, swallowed (it appears from his after statement) nearly two ounces of chloroform. I saw him at 10½ P. M., ten or fifteen minutes after he had taken the poison. An empty vial lying near had evidently contained chloroform, of which substance his breath smelled strongly. He had already vomited, but could not be roused from insensibility; breathing stertorous, pulse about 60. After an ineffectual attempt to administer an emetic, the stomach pump was used, and a pint and a half of warm water was twice injected into the stomach, and twice pumped out, loaded with the fumes of chloroform. Spirits of ammonia, ℥ij, were introduced into the stomach before the withdrawal of the tube. After removing the tube vomiting occurred, but the pulse became more feeble, breathing slow; eyes insensible to light. The effect of the cold douche on the head was tried for a few minutes with seeming advantage, the pulse growing stronger, but falling again, the application was discontinued. The surface becoming cold, he was wrapped in five or six blankets, a large stimulating enema administered, and resort had to mustard cataplasms. For the next hour (between 12 P. M. and 1 A. M.) the symptoms were more unfavourable; respiration very slow and feeble; face purple; pulse reduced to about 40, feeble, sometimes scarcely discernible, and intermittent. He evidently grew worse.

At 1 o'clock a gradual improvement commenced, and by 2 A. M. the respiration was easy, pulse 60, feeble but regular; insensibility continues, from which he did not recover till 5 A. M.

April 14. At 8 A. M. pulse 98, burning thirst; tongue coated; fauces red; pain in the stomach, which rejects everything. Says the vial (a two ounce one) was nearly full. Blister to epigastrium. There having been no operation from the bowels, sulph. magnes. ℥j was administered, but vomited. Enema, ol. terebinth. ℥ss, and ol. ricini ℥ij, in a large quantity of warm

water, brought away large stools. In the evening, milk was rejected by the stomach, likewise tr. opii gtt. xxx.

15*th*. Did not sleep; incessant short hacking cough; pain in the throat continues; pain is felt in the whole abdomen; tongue dry and thickly coated; stomach retains cold water but rejected ol. ricini ℥j. An enema caused several stools. Warm fomentations applied to abdomen, and at bedtime ext. hyoscyami gr. iij to be given.

16*th*. Passed restless night; pain in the region of the liver; jaundice appearing; pulse 110; skin hot and dry; expectoration somewhat rusty; no crepitation in lungs, but respiration harsh behind. Mass. hydrarg. gr. x; half dozen cups over region of pain. Tea and toast and milk were relished; several operations from the bowels during the day; jaundice increased by evening. Ext. hyoscyami gr. iij at bedtime.

17*th*. Pulse 96; skin soft and moist; pain in throat and over liver disappeared, and feels much better; conjunctiva and skin very yellow. Tartrate of soda and potash ℥ss.

18*th*. Slept well; pulse 70, soft and natural; continues to improve; jaundice decreasing.

After this, in consequence of pain in the right shoulder and region of liver at different times, cups and blisters were applied over the liver, and mercurials administered. By the last of the month he was well, and only waited, to be returned to duty, for the healing of the slight gun-shot wound.

Fort Mason, Texas, *May* 1, 1857.

FISKE FUND PRIZE ESSAY.

ART. XI.—*What are the Causes and Nature of that Disease incident to Pregnancy and Lactation, characterized by Inflammation and Ulceration of the Mouth and Fauces, usually accompanied by Anorexia, Emaciation, and Diarrhœa; and what is the best mode of treatment?* By DAVID HUTCHINSON, M. D., of Mooresville, Morgan County, Ind. The Dissertation to which the Fiske Fund Prize was awarded, June 3d, 1857.[1] (Published by request of the Rhode Island Medical Society.)

THAT a disease of this character has of late years existed to a great extent, especially in the Western States, and has excited considerable interest among physicians, is evident from the frequent notices taken of it by Western journals. But it is not a disease of recent origin. Marshall Hall has admirably described some of its symptoms. Abercrombie, in his inimitable work on the stomach and bowels, details a case, which, from the description, appears to have been of this character. In the lectures of Stokes and Bell, it is styled stomatitis nutricum; in Wood's Practice, nursing sore-mouth; and more recently by journal writers, stomatitis materna. Dr. E. Hale gave a description of it, to the Boston Medical Society, in 1830, and Dr. Bakus in the *American Journal of the Medical Sciences* in 1841. Since that time, several short essays on the disease have appeared in the medical journals of the United States. The disease has become more frequent, more closely observed, and better understood; but the literature of the subject is, as yet, scanty. Dr. Shields, of New Albany, Indiana, makes the statement (on the authority of the editor of the *American Journal of the Medical Sciences*), that neither the late Dr. Dewees, of Philadelphia, nor any of the physicians of that city, had ever seen a case of it, but that it was observed by Dr. A. Clapp, of New Albany, as far back as 1825; thus confirming the statement that the disease prevails more in some localities than others. That it is not peculiar to this country is evident from the writings of Abercrombie. That

[1] The Trustees of the Fiske Fund, at the annual meeting of the Rhode Island Medical Society, held at Providence, June 3, 1857, announced that they had awarded to the author of the dissertation bearing the motto—

"*Wheat from the fields of science, and cockles from my own farm,*"

the premium of one hundred dollars, by them offered for the best dissertation on the following subjects, viz: "*What are the Causes and Nature of that Disease incident to Pregnancy and Lactation, characterized by inflammation and Ulceration of the Mouth and Fauces; usually accompanied by Anorexia, Emaciation, and Diarrhœa; and what is the best mode of treatment?*"

And upon breaking the seal of the accompanying packet they learned that the successful competitor was David Hutchinson, M. D., of Mooresville, Morgan County, Indiana.

ISAAC RAY, M. D., Providence,
JAS. H. ELDRIDGE, M. D., E. Greenwich,
CHAS. W. PARSONS, M. D., Providence,
Trustees.

S. AUG. ARNOLD, M. D., *Secretary of the Fiske Fund.*

we may answer the first question propounded, "What are the causes and nature of this affection, incident to pregnancy and lactation?" we will first point out the symptoms and course of the disease, together with the anatomical lesions discovered by post-mortem examinations.

Although it is conceded by all writers on this disease that the affection of the mouth is only symptomatic of a previous pathological condition of the general system, yet the affection of the mouth is usually the first symptom that awakens the patient's attention. It generally comes on very suddenly, and, although she may have previously suffered from anorexia and various gastric disturbances, yet neither the physician's nor patient's attention is attracted to her condition till after the buccal symptoms ensue.

Dr. Byford (*American Journal of the Medical Sciences*) makes three varieties of this disease, taking the affection of the mouth as the basis of his division. These varieties differ from each other only in grade or severity; all pointing to the condition of the general system and the intensity of the disease. The affection of the mouth is but a part of the many forms that this protean disease assumes, for, as we shall see in the sequel, the disease is migratory, and spends its force on all the mucous surfaces. It is therefore most appropriate to treat of it according to its intensity or grade, and divide it into the acute and subacute or chronic forms.

The acute form generally makes its appearance either immediately before or shortly after delivery; the subacute some weeks or months afterwards. In both forms various gastric derangements, such as acid eructations, pyrosis, costiveness, alternating with diarrhœa, generally precede the affection of the mouth. In the acute form, the mouth affection generally begins with a scalding sensation, extending from the mouth down the œsophagus to the stomach, accompanied by a profuse discharge of hot burning saliva, difficult and painful deglutition; warm and hot fluids cannot be borne; there is loss of appetite and taste; tenseness and increased frequency of pulse; tongue red around the edges, and in patches on the dorsum. The redness speedily spreads throughout the whole cavity of the mouth and fauces. The parts are of a scarlet red colour, but as a general thing, not much, if at all swollen. This appearance may be of transient duration, lasting only probably for a few hours, more generally for several days, when it disappears, to be again removed in a very short time. But more generally, with this diffuse inflammation, the inside of the cheeks, gums, and under part of the tongue become covered with a crop of aphthæ or vesicles, which burst and become ulcers. The duration of this crop of vesicles and ulcers is generally about eight or ten days, and sometimes longer, when they will frequently disappear, to be succeeded by another crop of vesicles and ulcers as distressing as the former. In some cases the whole force of the attack is concentrated on the tongue, either on its side or under part; and begins from an inflamed point, or a fissure, or the bursting of a vesicle, which rapidly ulcerates, and spends its whole force on the one part, until the tongue becomes almost half amputated by a ragged notch. Suddenly this ulceration ceases, the cavity granulates, fills up, and heals; but the organ is left distorted. The whole force of the paroxysm is concentrated on this one point; the constitution suffers less than when the ulcers are more numerous and diffused over the buccal cavity. The disease would seem to have located itself, and its consequences are less to be feared, for when the stomatitis is more general, the inflammation is more disposed to extend itself along the adjacent mucous surfaces to all the neighbouring cavities. We have seen it travel from the mouth down through the pharynx and œsophagus to the stomach, thence through the alimentary canal its whole length; most

usually locating itself in the colon, and spending its force on that extensive mucous surface, and frequently destroying the patient by ulceration of its coats. Again we have in several instances known it pass through the larynx, trachea, and into the bronchia, and either establish inflammation in some portion of these tubes, or awaken into action disease of the lungs. It sometimes follows the nasal passages into the different cavities of the skull and maxillary antrum, and there induces permanent inflammation; or it may pass through the Eustachian tube to the tympanum, and thence to the mastoid cells. Dr. Byford states that he had seen a case where permanent deafness of one ear, and exfoliation of bone occurred. It also attacks the mucous surfaces of the vagina. Dr. Brainard had seen cases in which ulcerations of the vagina, alternated with diarrhœa. I have also observed cases in which ulcers of the mouth alternated with diarrhœa and vaginal ulcerations. Indeed, there is not a mucous surface but is liable to be attacked with this ubiquitous inflammation.

In addition to the local symptoms presented by the mouth, fauces, &c., derangement of the digestive and assimilating organs claims our first and earliest attention. As before remarked, the affection of the mouth is usually the first symptom that awakens the patient's attention; but nevertheless, derangement of the stomach and bowels always exists for some time prior to the occurrence of the buccal symptoms. I have invariably found that the greater the gastric derangements, the more extensive and difficult to heal were the ulcerations of the mouth. For weeks—it may be for months—before any affection of the mouth supervenes, the patient is much troubled with flatulent and acid eructations. She has a sinking feeling, a sensation of weakness at the epigastrium, or else a burning and feeling of distension, which requires her to loosen her dress to relieve the epigastric uneasiness. She also has slight febrile irritation, especially in the evening, with increased tenseness and frequency of the pulse. An acid fluid is generated in the stomach, and it would appear that everything the patient eats takes on the fermentative process; occasionally vomiting occurs; the bowels are either confined or too loose, and we would readily infer that the patient laboured under dyspepsia in one of its many protean forms.

Accompanying these symptoms are those presented by the condition of the urine and urinary organs. I found, on questioning my patients, that before the accession of a paroxysm, either of the sore-mouth or diarrhœa, that they laboured under painful and difficult micturition, which at times became distressing, and prevented sleep at night. They would pass only small quantities of urine at a time, not more than a tablespoonful or two, which generally attracted their attention from the highness of its colour, and amount of the deposit it threw down on standing. Sometimes this condition of the urine and urinary organs exists for several days before the accession of a paroxysm either of the sore-mouth or diarrhœa; for let it be borne in mind, that when diarrhœa ensues there is but little inflammation of the mouth; and again, when the mouth becomes inflamed, the bowels are generally confined. And I always found that the urinary difficulty was in a proportionate ratio to the intensity of the paroxysm either of the diarrhœa or sore-mouth.

I have examined the urine chemically and with the microscope in quite a number of cases. The following conditions were universally present:—

1. It is highly acid, instantly changing blue test paper to a deep red.
2. It is above the normal specific gravity, varying from 1024 to 1030.
3. The deposit thrown down on standing is urate of ammonia, and urate of soda, as revealed by frequent microscopic examinations in every case that

came under my care. In one case, a lady at the sixth month of utero-gestation applied to me for the painful and scanty micturition. She had the sore-mouth in a previous pregnancy and lactations, in a very severe form. She again had tenderness of the mouth and gums, and slight redness of the tongue, and occasionally diarrhœa. She said that she knew that the sore-mouth was about to come on from the urinary difficulty. She described the urine as passing only in small quantities, very red, and containing a copious sediment. I procured four ounces of the urine, which was very acid, and of high specific gravity, 1030. Under the microscope the figures of urate of ammonia, in the globular form, were very numerous but very small. There were also uriniferous tubules and epithelial cells. Dr. Lockhart, a very respectable neighbouring practitioner, and likewise a microscopist, had a number of cases under his care. He examined the urine in seven cases, and his observations correspond with my own. In all at some stage of the disease severe pain was felt in passing water, doubtless on account of an excess of acid (uric acid or its salts, urate of ammonia or urate of soda); such being the fact in the cases tested. In one of his cases the disease persisted for eight months, and, during the latter part of the term of her sickness, albumen was found in the urine in connection with epithelial cells, altered blood globules, and uriniferous tubules.

To prove the correctness of the microscopical observations, I subjected the urine to the test of heat, and when it reached the boiling point the sediment was dissolved, and smelled ammoniacal; thus proving that the deposit consisted of urate of ammonia.

Among the formidable array of symptoms which attended this affection, none is more distressing than the diarrhœa, which rapidly exhausts the patient, and produces emaciation, so that she becomes of a pale earthy colour. The discharges from the bowels are generally liquid, and in cases that persist for any length of time, mucus is often present; and when the case is about to terminate fatally, they are frequently tinged with blood. There are also colic pains and tenderness of the abdomen in cases of this description.

In the subacute form the patient is not troubled with so much intestinal disorder; she loses flesh and strength; the countenance becomes pallid; one or two ulcers are generally found about the mouth and tongue; there is a peculiar sense of weakness at the epigastrium; the bowels are usually costive, and although she suffers from flatulence and indigestion, yet the appetite remains tolerably good. We have known this form to persist during the whole period of lactation, and the patient still attend to her household duties.

Anatomical Lesions.—In the few dissections that have been made of patients that have died of this affection, ulcerations of the mucous membrane of the intestinal canal have existed in every case. Dr. Hubbard, of Ashtabula County, Ohio, saw at a post-mortem of a well-marked case, five ulcers, without any other morbid appearance to account for the fatal result. The buccal aphthæ preceded by several months the diarrhœa of which the patient died. The ulcers were circular, about three lines in diameter, indurated, and very deep. Three of them were situated in the colon, and two in the ilium; the surrounding surfaces were healthy, or nearly so. In the *Transactions* of the Indiana State Medical Society for 1856, Dr. McLean relates the post-mortem of a case in which the mouth and fauces were entirely denuded of their mucous coat, with numerous patches of ulceration extending throughout the œsophagus. The stomach was also almost completely denuded of its mucous coat, with numerous patches of ulceration extending deep into its muscular tissue. A small space around the pyloric orifice was the only healthy por-

tion. The duodenum was healthy. There were a few inflammatory patches in the colon. The bladder had traces of inflammation around its neck; and a few patches of ulceration existed in the vagina.

The following morbid appearances were found in a case which for a series of years was under my observation: In January, 1851, the patient was attacked, two weeks before delivery, with this affection, in a very severe form. Under treatment it disappeared, and was renewed a few days after confinement. It disappeared again, and was renewed afresh in two more weeks. The discharge of saliva was very profuse, the ulcers in the mouth numerous. She had hectic and night-sweats, and sank very low; doubts were entertained of her recovery, but under a tonic treatment she did recover, and continued to nurse her babe. She became again pregnant, and during pregnancy was threatened with the disease. She again used a tonic and nutritive treatment, which was continued during the latter months of gestation; and although there were frequently symptoms of the disease, yet by the treatment it was kept in subjection, and she continued to nurse her child. In 1854 she was again pregnant, and suffered from indigestion and stomach derangements. About the sixth month diarrhœa came on, which persisted in despite of judicious treatment, and she died three months after delivery, the case terminating fatally by convulsions.

I have been thus particular in the narration of the case on account of the extensive local lesions.

The autopsy was made seventeen hours after death.

The peritoneum exhibited evidences of inflammation; general appearance pink colour; bloodvessels of injected.

Adhesion of pancreas, throughout their whole extent to duodenum.

Structure of kidneys softened, congested, and pus in pelvis of; greatest quantity in right one.

In ureters, traces of inflammation one and a half inch in extent.

Want of integrity of colon; mucous membrane wanting in many places, the ulcerative process being so complete.

Cæcum as colon; small intestines and stomach for the most part normal.

Spleen completely softened, but for its serous covering would barely hold together; small collections of pus throughout its interior.

Liver enlarged; softened in the inferior portion of the right lobe, which was also congested, and showed signs of recent inflammation. A melanotic tumour, three-fourths of an inch in diameter, was found on its convex surface. Gall-bladder filled with black grumous material, about a gill in quantity. Weight of liver four pounds and two ounces.

In this case the patient was never free of the disease since the first attack in 1851, and although in the last illness the mouth was but little affected, yet it was evident that it was the same affection that had located itself in the colon; for whenever the diarrhœa would cease a few days, the buccal symptoms would appear.

Causes and Nature of.—The causes may be divided into two kinds, extrinsic and intrinsic, or remote and proximate.

1st. *Extrinsic Causes.*—Dr. Dunglison makes the remark that the disease prevails more in some localities than others. There are many practitioners that have never seen a case of it, while others have frequently had to contend with its intractable character. The disease is either of recent origin, or it has, to a great extent, escaped the observation of our predecessors; at least, during the last few years, it has attracted the attention of the profession. In some localities, it has been almost endemic among pregnant and nursing

females. Why is it that, during the last few years, it should have become more frequent? It was rarely seen in the Western States until after the appearance of dysentery and diarrhœa, in 1849, '50, '1, '2, '3 and '4. During the ten years preceding 1849 but two cases, of a mild character, came under my observation. After the appearance of dysentery, the affection became more common. Indeed I found, in many instances, the affection of the mouth associated with dysenteric symptoms. In those years there existed an epidemic constitution of atmosphere, producing a proclivity to disease of the mucous membranes. Indeed, during those years aphthous stomatitis was not an unfrequent accompaniment of dysentery in its latter stages; and in several instances cancrum oris supervened in children. Hence we infer that an epidemic constitution of atmosphere, producing a proclivity to disease of the mucous membranes, especially of the intestinal canal, is one of the extrinsic causes of this affection, peculiar to pregnancy and lactation. Since dysentery and other diseases of the digestive tube have been less frequent, we have seldom seen this affection, and during the last two years have been rarely called to treat it. Its prevailing more in some localities than others may be attributable to surrounding circumstances, as the quality of the soil, bodies of stagnant water, &c. Dysentery and diarrhœa have prevailed more in some localities than others, and as we have marked a coincidence in the prevalence of the two affections, we may attribute their prevailing in particular localities to similar causes. We are, however, fully convinced that the malaria that produces remittent and intermittent fevers is not a cause of this disease; for, when remittents and intermittents prevail, this affection is hardly ever seen.

2d. *Intrinsic Causes.*—These are to be sought for in the diathesis and constitution of the patient, and in the changes produced on the system by the functions of gestation and lactation. Those that are usually attacked with this affection are of a feeble, delicate constitution, and have previously suffered from debilitating causes, such as hemorrhages, leucorrhœa, and are generally either of the scrofulous or tubercular constitution, and labour under debility and anæmia. Young women of this constitution are frequently the subjects of it in the first pregnancy; and I have observed that frequent pregnancies and lactations are the most common sources of this affection. The most inveterate case that I have ever seen, the autopsy of which was given in the preceding part of the essay, gave birth to eleven mature children in seventeen years. Stomatitis, in a severe form, appeared with the ninth child, and her system never recovered from its consequences.

Youth may be regarded as a cause. Doctor Lockhart has given me some interesting statistics on this point. In twelve of his cases, the disease appeared in four at the third child, the ages of the mothers being respectively 24, 26, 22, 23. In six with the first child; ages 17, 20, 22, 20, 21. In two with the fourth child; ages of each 25. The cases reported by Brainard, in the *Northwestern Medical and Surgical Journal*, were all young women. My own cases were young females that procreated rapidly, and whose constitutions were illy adapted to the task of utero-gestation and lactation.

Another intrinsic cause is the changes effected in the system by gestation and lactation.

Dr. Wood says that the cause of this disease is some influence exerted on the system by the advanced state of pregnancy and lactation; what is the nature of this influence is unknown. Although pregnancy cannot be said to be a pathological condition, yet in certain constitutions that are inadequate to support the demands that are made on the system by that function, we find more or less general derangement ensues, which is produced by the new pro-

cess of development that is going on in the gravid uterus. In the early stages of pregnancy, the stomach sympathizes with the gravid uterus. Nausea and vomiting ensue, which deprive the female of sufficient nourishment for her own system. Depraved appetite is a frequent accompaniment. The sensibility of the nutritive functions are changed, she emaciates and becomes thin, the blood becomes impoverished, and it is more than probable that the blood impoverishment is the source of the disease, the unknown influence of which Dr. Wood speaks. Andral and Gavarret analyzed the blood of thirty-four pregnant women. In thirty-two of the cases they found the red globules below the healthy mean standard, in six of which they ranged from 120 to 125 in 1000, and in twenty-six from 95 to 120. They also found that for the first six months the fibrin was below the natural quantity, varying from 1.9 to 2.9, while during the last three months it exceeded it, varying from 2.9 to 4.8, and averaging nearly 4. Becquerel and Rodier analyzed the blood of nine pregnant women, viz.: one at the fourth month, five at five months and a half, one at six, and one at seven months. The maxima, minima, and mean results are given in the following table, copied from *Simon's Chemistry*:—

	Mean.	Max.	Min.
Density of defibrinated blood	1051.5	1055.1	1046.2
Density of serum	1025.5	1026.8	1023.6
Water	801.6		
Fibrin	3.50	4.	2.5
Albumen	66.1	68.8	62.4
Blood corpuscles	111.8	127.1	87.7
Extractive matter and salts	6.6	8 7	4.7
Fat	1.922	2.519	1.158
Consisting of serolin	variable	0.108	0.018
Phosphorized fat	0.646	0.863	0.381
Cholesterine	0.061	0.225	0.030
Saponified fat	1.195	1.323	0.737
The salts in 1000 parts of blood consisted of			
Chloride of sodium	3.2	3.9	2.3
Other soluble salts	2.4	2.8	1.8
Phosphates	0.425	0.690	0.282
Iron	0.449	0.490	0.370

From these analyses they conclude that pregnancy exercises a marked influence on the composition of the blood. The density both of the defibrinated blood and of the serum is diminished, the water and the phosphorized fat[1] are increased, while the corpuscles and albumen are diminished, thus showing that pregnancy impoverishes the blood.

Mr. West says that during pregnancy, even in a healthy woman, certain changes in the blood (a diminution of its red particles, and an increase of its watery elements) are of constant occurrence; while, in some instances, those changes are so considerable as to give rise to disorder of the general health, precisely similar in all its characters to chlorosis. The growth of the womb, the development of the fœtus are, indeed, accomplished, for they are subject to a law not easily broken through; but they are accomplished at the expense of the woman's constitution, and leave her often incapable of suckling her infant, and probably liable to all that class of inflammatory affections, the remote cause of which (as, for instance, phlegmasia dolens) is to be sought for in the blood. M. Cazeaux, who has patiently examined this condition of

[1] Phosphorized fat is always abundant in impoverished blood.

pregnancy, says that hydræmia or serous polyæmia is the most frequent cause of the functional disturbances that take place in pregnancy, and which are usually attributed to plethora, the analyses of the blood of pregnant women exhibiting a diminution of globules and an increase of water, differing only from chlorosis by containing an increased quantity of fibrin. And he further remarks that the functional disturbances of pregnancy resemble those of chlorosis, and that the effect of treatment confirms this view; that it is from the employment of iron and animal food that real benefit is obtained. M. Jacquemier, who has paid considerable attention to this subject, states that all the analogy that exists between chlorosis and pregnancy is that, after the middle period of pregnancy, women exhibit the commencement of anæmia; and from the examination of the blood of two hundred women in the eighth and ninth months of pregnancy, he found a diminution of the corpuscles, but not to the same extent as in chlorosis. And, among many hundred women auscultated at the Maternité, he only met with the carotid souffle in two or three. Andral says that when the corpuscles are below 80 in the 1000, that the *bruit de soufflet* is a constant phenomenon. It is often heard when their cipher oscillates between 80 and 100, and becomes more rare when the physiological mean is approached, and when it is reached ceases altogether. Mr. Williams says that the *bruit de soufflet* is often present in pregnant women, and corresponds with the frequent diminution of the red corpuscles in them. Other authorities might be cited to show the altered condition of the blood in pregnancy, amongst whom is Tyler Smith. Indeed, it is a fact well established that the red corpuscles are diminished and the fibrin increased. The buffy coat of the blood of pregnant females is a fact familiar to every one, and hence there exists a condition of blood in pregnancy which renders the system liable to inflammations; and we infer that this condition of blood induced by pregnancy is the intrinsic fountain of this disease incident to pregnancy and lactation.

Another probable intrinsic cause from our observations, is disease of the spleen. The most intractable cases that we have seen had this accompaniment. Disease of the spleen produces a cachectic condition of system, not unfrequently accompanied with ulceration of the mouth. In one of our cases the affection of the mouth, which alternated with diarrhœa, persisted till after the subsidence of the splenic affection.

Pathology and Nature.—At an early period of pregnancy, the stomach sympathizes with the gravid uterus; the new function that the uterus has to perform in the development of the fœtus, through its nervous connection with the stomach, produces various sympathetic disturbances of the latter organ. Nausea, vomiting, diarrhœa, anorexia, acidity of stomach, and pyrosis, are the frequent results of these extensive sympathies. Hence nutrition becomes imperfect, and consequently there is a diminution of the nutritive materials that supply the blood to repair the waste of the tissues. An almost poisonous influence (says Tyler Smyth) seems to be exerted by the gravid uterus in some constitutions, consequently the blood becomes impoverished from the impaired nutrition produced by the deranged digestive organs. And when we consider that the subjects of this affection have at best but feeble digestive organs, the chyle globules must be imperfect, and consequently the blood globules imperfect. We hence see that the function of nutrition is inadequate to the demands of the system; more especially as there is a new function to perform by the blood, not only in the nourishment and development of the fœtus, but that of the uterus and appendages also. In the unimpregnated state the uterus is a very small organ, while during the period of

utero-gestation it attains to an astonishing size, its capacity being increased a little more than 519 times, and its solid substance in the ratio of twelve to one; its bloodvessels are much enlarged, and many vessels that previously were impervious to red blood, now circulate it freely. For the development of the fœtus, uterus, and appendages, a heavy draught is made on the blood, especially in the latter months, when the increase of the fœtus, uterus, and appendages is much more rapid than in the early months of utero-gestation. There exists of necessity an increased consumption of red corpuscles and an increase of fibrin from the metamorphosis that is going on in the uterus. Simon, in his chemistry of man, gives us some interesting facts on this subject. He says that Denis made an analysis of the blood of the mother and of the fœtus; he found that of the fœtus richer in solid constituents and in blood-corpuscles than that of the mother. The corpuscles in the blood of the mother were 139 parts in the 1000, while of the fœtus 222; also the quantity of iron was greater in the blood of the fœtus than of the mother, the proportion being in the ratio of 1 to 2.5. Also the mass of the blood of the fœtus increases in a very rapid ratio with its development, the proportion of corpuscles is greater, and the quantity of water less, than at any subsequent period of life; and for some time after birth the proportion of corpuscles and of iron is above the ordinary standard. The facts, to a great extent, account for the blood impoverishment, and also for the fact that always attends cases of this affection, that the children of females who suffer from it are generally large, and what might be called well nourished. We thus infer that there exists a disproportionate ratio between the demands that are made on the blood by the function of utero-gestation and the nourishment of the system to supply those demands. After parturition, the drain on the already impoverished blood is kept up by lactation. And here again we find that the researches of Simon and other chemists throw much light on the influence of lactation on the female system. From their analyses we find that the solid constituents of woman's milk range from a third to one-fourth per cent. of the whole fluid; the salts having iron in their composition, besides butter, casein, and sugar of milk. We further find that the solid constituents of milk increase with the increasing age of the child. Simon gives fourteen analyses of the milk of one woman, made from the 31st of August to the 4th of January, a space of a little over four months, during which time the solid constituents of milk varied from 86 to 1.38, 6, the highest amount of solid constituents being on the 31st of December, and the lowest on the 11th of November. It is however to be remarked, that the solid constituents of the milk must and do vary with the quality and quantity of the food taken by the mother. We have always found that in this affection of pregnancy and lactation, that the lacteal secretion is abundant and of rich quality; and the infants, from their size and plumpness, gave evidence of such being the case. Hence, we infer that the impoverished state of blood produced by pregnancy is continued by lactation. We are therefore led to regard this affection of pregnancy and lactation as an inflammation—*sui generis*—of the mucous surfaces, produced and continued by an impoverished condition of the blood; which inflammation takes on the ulcerative process, the previously imperfectly nourished tissues presenting but feeble resistance to the ravages of the inflammation. We find in this disease a similar condition of blood to that which occurs in other inflammations, such as acute rheumatism, phlegmasia dolens, and other diseases as tuberculosis, &c., viz: a diminution of blood-corpuscles, and an increase of fibrin. We might cite numerous authorities to establish this point. M. Paget shows the similarity in appearance

in the red blood cells, in acute rheumatism, and in pregnancy. In short, the chemical constituents of the blood in pregnancy are such as is known to exist in many inflammations. Rokitansky speaks of an aphthous variety of fibrinous crasis of the blood, which gives rise to the exudations of muguet, diphtheritis, some dysenteries, and of hospital gangrene.

The fibrinous condition of the blood exciting the inflammation, and consequent ulceration of the tissues of the mucous surfaces, and the diminished red corpuscles determining the character of the inflammation attendant on this affection incident to pregnancy and lactation. The influence of treatment bears us out in this view; the condition of the urine is to the same point; its specific gravity is above the normal standard, and loaded with waters, showing that a great waste of the tissues is going on. All these changes are in accordance with what we know of some inflammations. Hence, by the changes in the blood we account for its migratory character. It is a well known pathological fact, that there exists a blood crasis in all diseases of a migratory character, although our means of research may as yet be insufficient to determine in what that crasis consists. Rheumatism is migratory, so is erysipelas. The researches of modern pathologists tend to establish their nature as consisting in the presence of a morbid principle in the blood. Andral and Gavarret, in their researches, show that the blood in rheumatism differs but little in the relation of corpuscles to fibrin from that of pregnancy; and from their analyses we find the same pathological relation of the corpuscles and fibrin in erysipelas. The course and symptoms of this disease incident to pregnancy and lactation, point to a blood crasis. Why else should it attack all the mucous surfaces in alternate succession? We have seen it suddenly make its appearance in the mouth, and as suddenly disappear and attack the mucous surfaces of the intestinal canal and vagina. Is it not more than probable that the aphthæ which appear in the mouth and intestinal canal, and degenerate into ulcers, and keep up diarrhœa, &c., may be a fibrinous exudation occasioned by the fibrinous condition of the blood. The influence of treatment brings us to such a conclusion, as we shall see in the sequel, that those remedies that decrease the fibrin of the blood are the most beneficial in this peculiar affection.

Recapitulation of the Views advanced.—1. Imperfect nutrition of the system, occasioned by the disturbed condition of the digestive organs, produced by sympathy with the gravid uterus.

2. An altered state of the blood, produced by pregnancy, and kept up by lactation. This condition of the blood being such as is known to exist in inflammations.

3. Its migratory character is dependent on the condition of the blood.

Diagnosis.—This affection may be distinguished from other forms of stomatitis by a burning sensation in the mouth, as if it had been scalded, which is greatly aggravated by hot drinks; attended at first with redness of the mouth and tongue, and followed by aphthæ and ulcerations of the buccal cavity. In some cases there is a diffused redness of the mucous membrane of the mouth, instead of ulcers. These symptoms are generally attended, and often preceded by a burning sensation in the stomach, pyrosis, indigestion, and occasionally vomiting. The bowels are either constipated, or obstinate diarrhœa attends. The disease is confined to pregnancy and lactation, although it has been said to attack those that were not in those conditions, and even the male subject; yet we have seen it in no other conditions but those of pregnancy and lactation, and would infer that it had been confounded with some other form of stomatitis. In addition to the foregoing symptoms,

its migratory character is highly diagnostic, and also the frequent and painful micturition which frequently precedes the affection of the mouth or diarrhœa.

Prognosis.—Always uncertain as to the final result. Although there is generally not any immediate indications of danger, yet such is the liability of the mucous structures to inflammation that the condition of the patient may always be considered precarious while the disease persists. When the disease extends to the larynx, trachea, or bronchial tubes, the patient may either die from the intensity of the inflammation, or at a remote period consumption ensues, usually in one or two years. But the patient is more apt to perish from the intestinal affection; when the diarrhœa persists, in despite of judicious treatment, and the discharges are mucous, tinged with blood, indicating ulceration of the bowels, a fatal result may be anticipated. In the subacute form, when the inflammation is confined to the mouth, and the patient measurably retains her strength, a favourable result may be expected. As a result of our observation but few recover a good state of health, unless they cease from the functions of utero-gestation and lactation.

Treatment.—From the views advanced of the pathology and nature of this disease, the indications of treatment are the following:—

1st. To correct and improve the digestive organs, and restore them to their normal functions.

2d. To correct the inflammatory condition of the system by supplying the blood with such nutriment and medicines as will augment the blood-corpuscles and decrease the fibrin.

3d. To remove the causes.

4th. To prevent the secondary affections, viz: the extension of the disease to those organs important to the continuance of life.

Indication 1*st.* For the fulfilment of the first indication we are possessed of a variety of means, some of which we will find adapted to the circumstances of the various cases as they may present themselves. As before remarked, we find that the greatest number of cases labour under acid eructations, gastric derangements, pyrosis, burning at the epigastrium, and either constipation or diarrhœa. To correct the acid condition of stomach, antacids and alkalies produce the most favourable influence; among the best of these is the bicarbonate of soda, which neutralizes the acid of the stomach, and at the same time the excess of acid in the urine. It may be frequently given during the day, either before or immediately after meals. In some cases I have found liquor calcis answer equally as well; it produces quite a soothing influence on the stomach and bowels given in the dose of a tablespoonful three or four times a day. Half a drachm of liquor potassæ, given in two or three tablespoonfuls of infusion of cascarilla bark, is also an admirable prescription, not only to correct the acid condition of the stomach, but also to improve its digestive powers. We have also used the bicarbonate of potass with good effects; it is especially useful when the bowels are constipated, from its laxative qualities. Alkalies answer the twofold purpose of neutralizing the acid condition of the stomach, and at the same time the acid state of the urine. The urine becomes more copious and clear after their use, and a diminution of the gastric disturbances and an evident improvement of the mouth usually follows. Alkalies are particularly valuable when the bowels are confined, but when diarrhœa ensues they have to be used with caution. They correct the acid state of the stomach temporarily, but do not prevent the generation of acid in the stomach. Consequently we must resort to such remedies as will prevent the secretion of acid by the stomach. For this purpose the vegetable and mineral astringents are the most efficient. Such as kino, logwood, catechu, krame-

ria, and bismuth, &c., all of which diminish the secretions of the stomach and intestinal canal. We have found the most beneficial effects from logwood, krameria and bismuth, an infusion of an ounce of logwood and a drachm of cinnamon in ten ounces of water and strained, of which from one to two ounces may be given for a dose before meals. We have tried kino and krameria, but prefer the logwood; patients take it more readily than either of the others, and its astringency is nearly the same. But to improve the digestive powers of the stomach, nothing is equal to the trisnitrate of bismuth in combination with an antacid. It not only soothes the mucous membrane of the stomach, probably by sheathing it from the acid generated by the stomach, but it also produces an anæsthetic effect on the sentient nerves of the stomach, which, in the indigestion that accompanies this affection, seem to be in a state of exalted sensibility. Bismuth restrains the secretions of the stomach, and also of the intestinal canal; it may be combined with columba powder with an admirable effect. Astringents are not only valuable in restraining the acid secretions of the stomach, which give rise to many unpleasant sensations in this affection, but they also serve to prevent the occurrence of that intractable form of diarrhœa that is so frequently an accompaniment of this disease.

Diarrhœa.—But few cases escape diarrhœa; the patient may have costive bowels one day and diarrhœa the next. The diarrhœa is but a transference of the disease from the mouth to the intestines; the acid condition of the stomach and intestinal canal producing irritation of the mucous membrane, inflammation, and an aphthous condition of the mucous follicles is easily set up, which terminates in ulceration. Dr. Hubbard, of Ohio, regards the disease as essentially ulceration of the mucous follicles, and relates seven cases in which the following treatment was eminently successful: ℞.—Sulph. zinc., pulv. ipecac, āā ℈j; pulv. mastiches ℈ij; terebinth. canadens. q.s.—M. Ft. mass pilul. in 60 dividend. The sulph. zinc. in the recipe is the only one of much medicinal value, the other articles rendering the pill insoluble till the zinc can reach the diseased follicles. Abercrombie cured his case, after a great variety of treatment, with a decoction of logwood. Byford has used with benefit the vinous tincture of golden seal (*hydrastatis canadensis*). Sulph. of alum and borax has also been used with benefit. We have found trisnitrate of bismuth and pil. plumb. acetas et opii of more efficacy in controlling the diarrhœa than any other remedies. We have used all the astringents, tannin, gallic acid, persesquinitrate of iron, with more or less benefit, but none have succeeded so well with us as the bismuth and pill of plumb. acetas et opii. The bismuth may be given in doses from five grains to a drachm, as often as may be thought necessary, according to the severity of the case. It is best adapted to those cases in which the diarrhœa is persistent, but moderate. How it acts is rather speculative, but it is probably by its tonic power on the exhalants of the intestines. There probably exists a loss of power of the terminal capillary membrane, resulting from the impoverished condition of blood failing to nourish the tissues of the system normally; consequently exosmosis of the fluids is easily produced. It likewise produces a tonic influence on the ulcerated mucous follicles. It has been known, when scattered on languid granulations on the cutaneous surface, to restore them to a firm and healthy character. It is perfectly insoluble, and in passing through the bowels comes in contact with the ulcerated patches, and thus produces a local astringent, combined with a tonic influence. It is most efficacious given in the dose of twenty grains three or four times a day, and that quantity may be given every two hours. It generally requires several days to insure the good effects of the remedy, but when its effects are produced they are more perma-

nent than from any other article. In cases in which the diarrhœa is copious, and seems to rapidly exhaust the patient, the pil. plumb. acetas et opii is more prompt than bismuth, especially when the stools contain mucus tinged with blood. The acetate of lead also restrains the secretions of the stomach and intestinal canal, and assists in cutting off one source of the diarrhœa. The pil. plumb. acetas et opii may be given in the dose of three grs. plumb. acet. to one-fourth gr. of opium every two, four or six hours, according to circumstances. During the continuance of diarrhœa, the patient ought to maintain the horizontal position, and abstain from every kind of food that may disagree with the stomach and bowels.

Indication 2*d*. To correct the inflammatory condition of the blood by introducing into the system such remedies as will increase the corpuscles and diminish the fibrin, bloodletting has long been a remedy for inflammation, but in this affection it is wholly inadmissible, unless under peculiar circumstances. In cases with tense full pulse, extreme pain in the mouth and jaws, bloodletting in one instance was productive of great comfort to the patient, but nothing but the severity of the pain ought to induce the practitioner to bleed. When the bowels are confined and aperients are required, none has proved so valuable as the saline. Salines lessen the acidity of the urine and decrease the fibrin of the blood, and it will be found that the inflammation of the mucous membranes are less when the urine approaches to the normal state. There exists, then, less acid in the stomach, and consequently one source of irritation is cut off. Of the salines, none has proved so valuable as the tartrate of potass and soda (the common Rochelle salts); it not only acts mildly as an aperient, but produces a rapid effect on the acidity of the urine. According to the researches of Bence Jones, 120 grs. of dry tartrate of potass, dissolved in four ounces of distilled water, will render the urine alkaline in thirty-five minutes.

Salines also dissolve the fibrin of the blood, and thus render it less inflammatory, and act as antiphlogistics; especially the salts of potash are antiphlogistic by dissolving fibrin. They excite the secretions, especially of the kidneys, and thus eliminate the morbid materials from the blood. The iodide of potass, which is a favourite remedy with many, acts on the same principle as an eliminator. It is, however, better adapted to the subacute or chronic form than the acute. Salines thus act as hæmatics or blood restoratives in this affection, and are more admissible than any other antiphlogistics.

To increase the blood-corpuscles and decrease the fibrin, cod-liver oil is of the first importance. To Professor Evans, of Chicago, belongs the merit of having applied this remedy to nursing sore-mouth. In the *Northwestern Medical and Surgical Journal* for 1853, he says that, "observing the influence of cod-liver oil in preventing the wasting of the tissues of the body in cases of marasmus, especially from phthisis and tabes mesenterica, it occurred to me that it might be equally beneficial in the disease in question. I have accordingly been in the habit of giving it in French brandy or malt liquor, as might be best suited to the taste or most convenient, and generally with the happiest effects. When the patient can be induced to continue its free use, it has generally proved beneficial, and, in most instances, effected a cure." Dr. Byford also speaks in terms of confidence in relation to the curative properties of cod-liver oil in this affection, and says that it must be persevered in during the whole term of lactation, and as long afterward as any trace of the disease remains. This remedy has the advantage of any other tonic and alterative from its soothing effects on the bowels. A case that came under my care was cured by the oil, bismuth, and bicarbonate of soda, after resisting

for weeks every other treatment. The patient was reduced low, and was unable to turn herself in bed. The improvement was evident from the commencement of the cod-liver oil treatment. It is frequently difficult to get the patient to persevere in the use of the cod-liver oil; the stomach will frequently revolt at it. To obviate this, it is exceedingly necessary to pay strict attention to keep up its powers by some mild tonic and antacid, either the tincture of gentian and bicarbonate of soda, or an infusion of cascarilla bark and liquor potassæ. Alkalies are useful adjuvants to the cod-liver oil, promoting its absorption into the blood; for it is essentially necessary that the oil be absorbed to produce its beneficial influence in changing the chemical constitution of the blood. The digestive powers in this affection are too feeble to elaborate sufficient fatty materials from the food to unite with the albuminous part of the chyme, to form healthy chyle globules, which subsequently become blood globules. Hence, the introduction of cod-liver oil into the system accomplishes what the weakened powers of the stomach were not able to do. Experiment has established the fact beyond a doubt that cod-liver oil increases the blood-corpuscles and decreases the fibrin. Dr. Snow, by his analysis, shows that the corpuscles are increased by cod-liver oil taken into the system. Also Simon, by his analysis of the blood of those using cod-liver oil, records an increase of blood-corpuscles and a decrease of fibrin, although from their analysis it appears to increase the blood-corpuscles in a greater proportionate ratio than it decreases the fibrin. Again, cod-liver oil, by increasing the blood-corpuscles and bringing the blood to a healthy condition, the various secretions and excretions from the blood will approximate nearer to a healthy state; and consequently the solid contents of the urine will be lessened when they were previously in excess. An example of this effect of cod-liver oil is well illustrated in the disease diabetes, in which the specific gravity of the urine is very high, from 1040 to 1060. In this disease cod-liver oil has been found to daily and increasingly diminish its specific gravity. We found the specific gravity of the urine high in this form of stomatitis. And if the opinion of Sherer be correct, that the colouring matter of urine is decayed blood-corpuscles, we must consequently see that cod-liver oil is adapted in more respects than one to this peculiar affection. But whatever may be its peculiar *modus operandi*, experience abundantly testifies to the fact that it is of essential value in this disease, peculiar to gestation and lactation. I generally give it in the dose of a tablespoonful three times a day, an hour or two after meals. Some take it best in brandy, others in the compound tincture of cardamoms, according to the taste of the patient.

The ferruginous preparations, from their well known quality of enriching the blood with red corpuscles, and from their adaptation to some cases of feeble and impaired digestion, would seem to be very applicable to this disease. Doctor Bakus used a combination of the carbonate, rhei, aloes, and ipecacuanha, in the form of pills. Doctor Byford uses the carbonate in the following manner: Take carb. potass., sulph. ferri, āā ʒiss; gum acacia mucilage ʒiv; pulverize the potash and dissolve in the mucilage, then pulverize and add the sulph. ferri, mix in an earthen mortar. Dose half an ounce three times a day, gradually increasing it to as much as the stomach will bear. He uses this only when there is no diarrhœa. I have found iron more valuable as a prophylactic, to prevent a recurrence of the disease, than as a curative agent during its continuance. Its action is too slow to be of much value during a paroxysm of this affection, but when used in conjunction with cod-liver oil for a length of time, its effects are very certain and permanent. In one case in which the disease made its appearance in the lat-

ter months of utero-gestation, I gave the protocarbonate of iron (Vallet's mass) and cod-liver oil, with the effect of entirely preventing a recurrence of the disease during lactation. The protocarbonate is a salt of iron that speedily finds access into the blood, and is quicker in its influence than the carbonate, which is nearly insoluble. Quevenne's iron by hydrogen is also well adapted to this condition. But when the tenderness of the mouth will admit, the syrup of the iodide is the quickest and the most efficient preparation. Iron, to be useful, must, like cod-liver oil, be persevered in for a considerable length of time, and the patient ought to be informed that unless she persevere with the remedies she need not expect relief. As a prophylactic, iron ought to be given so soon as the patient becomes pallid and begins to exhibit symptoms of anæmia. The sulphate of quinia is also a valuable tonic in this affection, in the dose of three grains twice or three times a day. It imparts power to the nervous energies of the system, promotes digestion, and exercises a controlling influence on the febrile irritation. When diarrhœa exists, I have found small doses of strychnia, combined with sulphate of quinia, of much value as a tonic, it exercising a controlling influence over the diarrhœa.

Local applications to the mouth have been found to be only temporary in their influence, and consequently of but little benefit in this affection. Nitrate of silver, hydrochloric acid, and various astringents have been used with but transient relief to the mouth. Even if the ulcers did heal under their use, another crop of vesicles soon appeared as bad as the former. Some practitioners have spoken very highly of the *Hydropiper punctatum*, with which I have had no experience, and presume that its beneficial influence, like all other local applications, is of but transient duration. So far as my experience extends, the best local applications have been nitrate of silver and dilute hydrochloric acid. I have found advantage from removing all carious teeth and salivary calculi from the mouth. Vitiated saliva from unhealthy teeth and gums passing into the stomach assists in keeping up the gastritic disturbances, to which patients with this disease are so liable.

Indication 3*d* *To remove the causes.*—A change of location has been known to result in a restoration to health, and ought to be advised when practicable. The weaning of the child has been advised by all who have written on this affection. When a wet nurse can be obtained, it is safest for the patient to cease from the function of lactation. Although it may seem repugnant to the feelings of the mother to deprive her of the pleasure of nursing her offspring, yet it becomes imperative when the disease persists in despite of judicious treatment. Although the children of patients with this affection are generally large, and apparently well nourished, yet when the disease persists for a length of time, they begin to droop and generally fall victims to bowel affections. To ascertain the cause of this bowel affection of children, I submitted the milk of one of my patients to several microscopic examinations, and although these examinations were not sufficiently numerous to advance any facts worthy of attention, yet I found that after the children began to deteriorate in health the milk, in addition to the healthy milk globules, had another globule, resembling in appearance the inflammatory globule of Gluge. I mention this as worthy of further investigation, and as bearing on the point whether it is not better for both mother and child that the mother should cease nursing. Although the cessation of lactation does not always cure, yet it puts an end to the drain on the mother's already impoverished blood; and thus, by cutting off one of the sources of disease, remedies are permitted to produce their beneficial influence without the interference of a counteracting

influence existing in the system at the same time. Hence it is best, in cases of any gravity, to advise the cessation of lactation.

Indication 4th. To prevent and counteract the secondary affections, or, in other words, the extension of the disease to organs important to the continuance of life.—To fulfil this indication the patient ought to be watched with the most sedulous attention; and so soon as either disease of the respiratory organs or of the alimentary canal manifest themselves, every effort ought to be made to counteract them, the treatment for which will be the same as that of other inflammations of these organs, bearing in mind the already impaired condition of the system. To prevent the supervention of phthisis pulmonalis, the most frequent sequel of this affection incident to pregnancy and lactation, a continuance in the use of cod-liver oil is advisable, even after the disease, to all appearance, has entirely subsided, together with strict attention to the digestive faculties, exercise in the open air, and an avoidance of all depressing causes.

In bringing to a conclusion an essay on a disease which has but of late attracted attention, and on which but little has been written, it may add to its value (if value it has) to append some illustrative cases, drawn from reading and observation. The first case I find on record is one by Abercrombie, in his invaluable work on the stomach and bowels. It is the 155th case, page 365, 2d edition, and is as follows:—

"A lady, aged 30, came under my care in the spring of 1830, affected in the following manner: She had a remarkable tenderness on the inside of the lips, the tongue, and the throat; a constant discharge of saliva, a burning uneasiness of the tongue, throat, breast, and stomach, and great uneasiness in swallowing, and for some time after it. She had a constant tendency to diarrhœa, and a feeling as if food did not remain in the stomach, but passed immediately through the bowels. There was some cough, with frequent pulse, great debility, and increasing emaciation. The throat appeared raw and a little inflamed, the edges of the tongue and the inside of the under lip were excoriated, and covered with small ulcers having inflamed margins. There was also a painful excoriation about the anus and the labia. The complaint was of three months' standing, and had begun while she was in the puerperal state in England. A variety of treatment had been employed without benefit; she became emaciated to the greatest degree; the diarrhœa became incessant, with much pain, and a feeling as if everything passed through her immediately. She had no relief but from large opiates, and that relief was but slight and temporary. When the case appeared to be hopeless, she began to take a decoction of logwood, one oz. to one lb. of water, a wineglassful four times a day, combined with a small opiate. From this time she recovered daily, and in two or three weeks was in perfect health."

This case shows the general character of this disease. It attacked the mucous membrane of the mouth, air-passages, stomach, and intestines, even to the extremity of the anus, and also the labia.

Doctor Brainard's cases also go to show the general effects of this affection. The following is one of his cases, which he describes as ulcerations of the vagina during lactation:—

"A woman, aged 35 years, had been affected for a long time with pain in the back, hips, &c., for which various remedies had been used without effect. On inquiry, he found the symptoms dated from the period of lactation, and were attended with debility. On examination, several minute points were seen about the orifice of the vagina, scarcely perceptible to the eye, but which,

when the surface was touched with a solution of lunar caustic, turned white, revealing the existence of numerous ulcerated points. The appearance of minute red points upon the mucous surface, of a pale colour, he had seen in other cases, and they are well calculated to deceive, unless a solution of nitrate of silver, of about twenty grains to the ounce, is passed over the surface." He adds: "It were easy to add to these cases others, where the ulceration of the mouth alternated with diarrhœa, indicating a transfer of the ulceration from the intestinal mucous membrane to that of the mouth, and *vice versa*."

The following cases occurred under my own care; one the subacute form of the disease, and the other the acute:—

Case I.—Mrs. W., while nursing her first child, had ulcers on the sides of the tongue; mouth very tender; child three months old, and was labouring under dysentery in a mild form; the dysenteric symptoms were relieved in a few days, after which she was placed on iodide of potass five grains three times a day, when the ulcers began to heal, and by a continuance in the remedy for a month she was entirely relieved, and continued to nurse her child. Frequent cases of a similar character came under my care, and were relieved by similar treatment.

The following is a case of the acute form:—

Case II.—In 1851, a lady that had the disease in the subacute form in a previous nursing; this time she had suffered much during the latter months of utero-gestation from gastric derangement, acid eructations, and costiveness, alternating with diarrhœa, scanty, high-coloured urine, with a frequent disposition to urinate, which was attended with smarting and burning sensations. While in this condition she was attacked two weeks before delivery with a scalding sensation in the mouth, extending down the œsophagus to the stomach, a profuse discharge of hot burning saliva, loss of appetite and of taste, tongue very red around the edges, and in patches on the dorsum. On the second day of her illness the pain in the jaws became distracting and intolerable; discharge of hot saliva from mouth very profuse; pulse full and tense; bowels constipated; urinary secretion very scanty. Twenty ounces of blood were abstracted from the arm, which gave her great comfort by relieving the intolerable pain in the mouth and jaws. She was given a saline draught, which brought away very dark fetid evacuations, and reduced the inflammatory condition of system. The inside of the cheeks, gums, edges, and under parts of the tongue became covered with small ulcers, with a red, fiery surface around them. Nitrate of silver, and also hydrochloric acid, as a lotion, were applied to the ulcers without any apparent beneficial effects. She also took iodide of potass for several days without benefit. The stomach continued deranged, acid eructations, and a hot burning sensation at the epigastrium. The iodide potass was laid aside, and she was given an ounce of aqua calcis three times a day, and saline aperients (tartrate potass and soda) to keep the bowels soluble, which corrected the state of the stomach; the urinary secretion became more abundant; and by the application of the nitrate of silver as a lotion, the ulcers began to heal, and the mouth speedily improved. A few days after delivery, the sore-mouth returned; the tongue assumed the same aspect as before; ulcers formed on its sides and under part, and on the inside of the cheeks; a febrile condition of system, of a hectic character, existed; much gastric derangement, acid eructations, and burning of the epigastrium; coated tongue, impaired appetite, and scanty, high coloured urine, with painful micturition. The urine, on standing a few hours, threw down a copious

precipitate. She was again given saline aperients (tartrate of potass and soda), aqua calcis, and the lotion of nitrate of silver to the ulcers. After five days' treatment the state of the stomach and bowels became corrected, and the state of the mouth again began to rapidly improve; and here I noticed, that the lotion of nit. argent. had no influence on the ulcers in the mouth until the condition of the stomach was corrected. In two weeks after she recovered from the second attack; the disease again returned; mouth exceedingly tender; edges and under part of the tongue covered with ulcers; tongue furred; scanty, pink-red coloured urine; throws down a heavy precipitate on standing; frequent pulse; hectic, exhausting nocturnal perspirations; loss of appetite; pyrosis; flatulence; discharges from bowels hot and excoriating; flow of saliva abundant; emaciated fast, and began to sink low. Aqua calcis was given for burning at epigastrium, without producing the relief it had done formerly, but the bicarbonate of soda was given with a very pleasant effect. My patient at this time had not been out of bed for several weeks, and with difficulty could turn herself in bed. At this juncture she was ordered cod-liver oil—a tablespoonful thrice a day in a little brandy—subnitrate of bismuth five grains thrice a day; quinine two grains thrice a day; solution of bicarbonate of soda as a drink *ad libitum*. The bismuth and soda acted almost as a charm in correcting the state of the stomach, so that from this treatment in four days she was much improved. The tongue and mouth lost their fiery colour, the ulcers began to heal, the nocturnal sweats ceased, the febrile condition of system became less, and the appetite and strength began to return. The treatment was continued three weeks, when she was quite restored, and continued to nurse a fine, healthy, stout boy. It required aperients to keep the bowels soluble. The salines appeared to produce the best influence, especially the tartrate of soda and potass; the urinary secretion being more abundant after their use, and a consequent improvement of the stomach and mouth. This lady fell a victim to the disease three years afterward. She died from ulceration of the bowels, accompanied with chronic diarrhœa.

I might add other cases to the above, of a somewhat similar character. It would probably extend this paper to a greater length than might be desirable. In the foregoing pages I have tried to faithfully record my experience in this perplexing affection, together with the train of thought that the nature and pathology of the disease had suggested to my mind. The following are the authors that I have consulted on this disease, viz: Wood's Practice; the Lectures of Stokes and Bell; Byford, in the *American Journal of the Medical Sciences;* Hubbard, in do.; Shields, in *Western Journal of Medicine and Surgery;* Evans and Brainard, *North Western Medical and Surgical Journal;* and my former essays in *North Western Medical and Surgical Journal* and *Western Lancet.*

REVIEWS.

ART. XII.—*The Treatment of Vaginal Fistula.*

1. *On the Treatment of Vesico-Vaginal Fistula.* By J. MARION SIMS, M. D., of Montgomery, Ala. (With twenty-two wood-cuts), pp. 24. From the *American Journal of the Medical Sciences* for Jan. 1852.
2. *Remarks on Vesico-Vaginal Fistule, with an account of Seven Successful Operations.* By N. BOZEMAN, M. D., of Montgomery, Ala. (With wood-cuts), pp. 29. From the *Louisville Review* for May, 1856.
3. *Urethro-Vaginal and Vesico-Vaginal Fistules—Remarks upon their Peculiarities and Complications, &c. &c.* By the same author. (With seventeen wood-cuts), pp. 23. From the *North American Medico-Chirurgical Review* for July, 1857.
4. *The History and Treatment of Vesico-Vaginal Fistula; a Report read before the Medical Society of the State of Georgia.* By P. M. KOLLOCK, M. D., Professor of Obstetrics in the Savannah Medical College. (With nine wood-cuts), pp. 32. Augusta, 1857.

NONE can have failed to notice the remarkable advance made of late by Obstetric Surgery. Within the century this department, aside from midwifery proper, itself then thought almost beneath contempt, was utterly unacknowledged by the profession. Now, on the contrary—though the legitimacy of its every operation, its every means of diagnosis, instrumental or manipulative, and of treatment, ligature, knife, suture, escharotic, compress, injection, are warmly—at times bitterly—contested—it has taken its place as an independent branch, distinct from General Surgery.

In this matter, as in others, general practitioners have been slow to acknowledge the claims of those who, by ill health, abundant worldly means, or ambition, have been enabled or compelled to devote themselves especially to it; although to such subdivision of labour (like that obtaining among lawyers and naturalists), as adopted and practised by physicians of honour, good education, and general experience, all our large communities are fast and willingly and advantageously tending. Impartial conservatism however, nor unfair opposition can longer withhold from obstetric surgery, unconfounded and ununited with midwifery, its honours as both science and art. Of all the triumphs, early and late, of this department, none excel, as few indeed of general surgery can equal, that which we are now briefly to discuss.

The rational, or at least the successful treatment of vesical fistulæ in women, dates back hardly ten years—up to which period many, probably most cases were pronounced, even by the best surgeons, incurable; cures where luck gave them, being gotten only by often repeated trial; while now "the surgeon can approach them with a confidence of success before unknown." We need not wonder that with the first approximation to this result was laid at once the foundation of an individual's world-wide fame, and of the Woman's Hospital of New York; well is it when, with so much public benefit, there is found a private one at all corresponding.

That these improvements are not overrated might easily be shown by quoting opinions most authoritative at their time :—

"If the opening is large, close it with a double stitch (the edges of the wound having been refreshed), keeping the flexible catheter in the bladder until it is entirely filled up. I wish this operation may not be found impracticable."[1] (Smellie, 1766.)

Mr. Liston (1828) publicly said :—

"It was seldom that union took place. All, indeed, might appear to go on well for eight or ten days; but at the expiration of that time the wound probably would be found to have been enlarged by having been interfered with, and would become larger and larger every time the attempt at cure was made."[2]
"When the communication is to a large extent, but little hope remains to the patient."
"In the majority of cases I fear we shall find but little benefit." "Indeed, vesico-vaginal fistula has long been considered as one of the *opprobria* of surgery, and, with some exceptions of late years, the cure has been given up as hopeless."[3] (Churchill, 1844.)

Of such assertions, many more could be given; but there is no need. They prove that the lesion was thought till of late almost absolutely and necessarily incurable. Even at the present day, professed and standard surgical text-books may be found, which pass over this "most distressing and intolerable accident to which females are subject" in silence.

Urinary fistulæ in the female, whether affecting the urethra or bladder, whether involving the vagina merely or the cervix uteri, and whether single or not, have presented to treatment an almost endless succession of obstacles, among which the difficulties of preliminary exploration and of operation were not alone; in the hands of the best surgeons and nurses, ligatures would cut and slough away, or urine would come dribbling through between them.

We shall follow these obstacles one by one, seeing how they have been successively met and overcome, rather than take up chronologically the various operations that have been proposed.

Preliminary Exploration.—It might be supposed that to ascertain the existence of a vesical fistula, is a matter very readily accomplished; this is not, however, the case. Far less is it easy, in many instances, to diagnose the size, position, and exact relations of the aperture in question, even when its existence is beyond all doubt. We were once called upon to operate for fistula, diagnosed as such by one of the first obstetricians now living in this country. On examination, the vagina was found large, unbridled, presenting none of the difficulties shortly to be considered, yet there was no fistula. The case was one of chronic and intermittent incontinence of urine, of many months' standing, undoubtedly identical in origin with the temporary form not uncommon immediately after delivery. Such a mistake might readily be made on casual or imperfect examination. In a vagina large, wrinkled, flabby, reeking with leucorrhea, and perhaps heated by chronic vaginitis, a stream of urine trickling upon the finger from a relaxed meatus might well give the idea of an abnormal passage, especially if that idea had already been entertained from the patient's description.

But it is as regards the character of an existing orifice, that most difficulties have been found. There are frequently contractions of the vagina, cicatrices, bands, which must be dilated or divided, their interstices and

[1] Midwifery, vol. i. p. 386.
[2] Lancet, June 23, 1828.
[3] Diseases incident to Pregnancy and Childbed.

angles giving passage to the urine, and to the touch all the characters of the fistula itself save one—a distinguishable transit through the vesical wall. Nor are these preliminary divisions always so easy as might be imagined. Frequently necessary very high up, and through a tissue tense and resisting, yet not perfectly to be governed under the knife, they offer all the dangers attending incision of the cervix for dysmenorrhœa or sterility, whether of peritonitis or pelvic abscess, and are not always so readily performed.

The position of the patient, seemingly so trifling a matter, is of the first importance. Originally it was thought best to put the patient on her back, as for lithotomy, the position recommended by Jobert, Kennedy, Hayward, Malgaigne, and almost every operator save Velpeau and Chelius, till Sims, in 1852, drew attention to its inconveniences. This surgeon, in advocating the change to the knees, has evidently fallen into an error concerning priority of the proposal. He claims that while he had chanced upon this plan in 1845, previous to the translation of either Velpeau or Chelius, its advantages had been perceived or made public by no other save themselves till after 1852. On the contrary, as early as 1840, Dr. Churchill, of Dublin, uses the following language:—

"I have found the knees and elbows far more convenient, and I think less offensive to the patient's feelings. The light can reach the part more readily, and the position of the operator is more convenient."[1]

Sims places his patients on an ordinary table, which is not found to answer every indication when an anæsthetic has been administered. Kollock has constructed a special table, with a movable stage—convenient, doubtless, in an operating theatre, but not easily carried from house to house. We have preferred in practice the following plan, suggested in all its details by a colleague, Dr. Nathan Hayward, of Roxbury. Nothing can be simpler, nothing more convenient. A common high-backed chair, or a small old-fashioned washstand, properly guarded by pillows, is placed on its face upon the bed; over its back the patient is made to bend, her arms extended and secured, her knees at a right angle strapped to the rounds or sides of the frame. She is thus immovably confined in just the posture needed, and the attendance of one or two additional assistants rendered unnecessary. To simplify the matter still more, the anæsthetic may be permanently placed under the patient's face on a cricket, or suspended there from the cross-bars of the frame, or, as in our actual practice, her nightcap may receive the sponge, and then be tied over her face.

Before operation, the fistula must have been brought into view, and room made for all necessary manipulation. Good light is necessary, best that of the sun, direct if possible, or reflected from a mirror as suggested by Sims. A careful exploratory examination must, of course, have been made, the size and position of the fistula have been accurately ascertained by engaging therein, and thus offering to the touch per vaginam, the point of a sound, or catheter, or bougie, or probe passed into the bladder by the urethra. All bridles and adhesions of the vaginal walls must have been divided by previous operation; and what none seem to have proposed or thought of, the vagina should have been dilated, unless already far more patulous than usual, to its utmost limits by huge sponge-tents—well shaped and properly placed, these will not increase the size of the fistula. The importance of another preliminary seems also to have escaped notice; by confining the patient to bed for a few days previously, as found so useful before many capital operations, and by putting her on full

[1] Diseases of Pregnancy and Childbed.

preparatory doses of ox-gall during this period, as insisted on so strongly by Clay, of Manchester, before ovariotomy, the tendency to subsequent unpleasant symptoms is greatly diminished. And finally, in those frequent cases where the urine inclines to profuse calcarious deposit, which might tend mechanically to irritate the wound, and thus, both directly and indirectly, to prevent its closure, it would be well to enforce the plan lately proposed by Kollock, and exhibit sulphuric acid internally for some days previous to the operation.

Next in importance, as in natural sequence, to the position of the patient and the access of light, is found the temporary dilatation of the vagina or repression of its walls; without thoroughly securing which, the operation can hardly be performed. A variety of modes of effecting this have been proposed; specula of various kinds, tubular, double-bladed, or resembling that for the rectum, as suggested by Montgomery; all of them affording so contracted a space for manipulation as to render it tedious, difficult, or impossible. Bent spatulæ, more or less in number, have been used, requiring the presence of several assistants.

The only instrument as yet suggested which at all answers the indication, is the admirable duck-bill speculum of Sims, as made by Otto and Kœhler, of New York, which by elevating and supporting the perineum, the patient being in proper position, thus opens the vagina to an enormous extent.

The Operation.—Cauterization of the fistula has had many advocates, both as a distinct and sufficient operation by itself, and for merely preceding suture, in place of the knife, for which last indication it cannot be too summarily condemned.

As an absolutely curative method in vesical fistulæ, the cautery has been greatly overrated. Hardly a case can be instanced where it has been perfectly successful. The fistula can easily be reduced in size by it, but seldom entirely obliterated, whether there be used Dupuytren's acid nitrate of mercury, the nitrate of silver, or the hot iron. Some experience of the two latter, and in Edinburgh of the electro-galvanic wire, as advised by Marshall and Middeldorpf, has been to us far from satisfactory.

The operations by suture have been many and diversified, all of them successively lauded, but most of them till of late have failed. Here, as in so many other operations, the simplest means have ultimately proved the best.

Each step in the process has given opportunity to surgeons for an endless succession of methods and complications. To depress the fistula, to refresh its edges, to replace protrusions of the vesical mucous membrane, the needle, to pass it, the ligature, to secure it, all have been battle grounds. The methods and instruments of Hobart, of Malagodi and Beaumont, of Schreger, Fabbri and Ehrmann, of Laugier, Lewzinsky and Colombat, of Roux, Deyber, Chassaigne, Desault and Dupuytren, down to the late ones of Mathieu and Baker Brown, bear witness only to the baffled ingenuity of those who proposed them. The serrefines brought forward last year by Bertet,[1] though so much more simple, prove as useless as the intricate leaf-clamps of Naegele and Lallemand.

Nor need more be said of the plan of Vidal de Cassis, for permanent closure of the vagina, an operation even now, perhaps, necessary in some cases where the cervix is involved, but in all others superseded and unjustifiable; nor for the same reason, of the plastic dissections of Jobert, Leroy d'Etiolles, and Velpeau, save to claim for the assertions of the former, which have gained for him such extensive reputation, their fair share of incredulity.

[1] Union Médicale, Aug., 1856, p. 375.

It is somewhat remarkable that most of the really important advances in the treatment of vesical fistulæ have been made in this country. Previous to the suggestions just alluded to, whose authors curiously enough were residents of the same place (Montgomery, Alabama), Pancoast, of Philadelphia, had secured union by dovetailing the raw edges of the fistula together; Hayward, of Boston, by so dissecting as to supersede the necessity of involving the mucous membrane of the bladder in the stitch, had insured a broad surface of adhesion and avoided a principal danger; and Mettauer, of Virginia, had substituted for other sutures, and secured them by twisting, threads of lead. To each of these gentlemen great credit is due for their several parts towards the simple and effectual operation now attained.

In January, 1852, there appeared in this *Journal* that proposal by Dr. Sims, now of New York, which for successful result will remain as it has been the greatest triumph of obstetric surgery. This operation, conceived in 1849, naturally succeeds those of Hayward and Mettauer, and in reality is based on a judicious combination of the essential points of those two methods. It consists of Hayward's stitch, down to but not perforating the mucous membrane of the bladder, made with Mettauer's metal thread, secured not by the twist, difficult of perfect adaptation and liable to become loose, but by immovable clamps to metal quills.

Perfect adaptation of the parts in most cases was thus secured, firm lateral pressure over a wide surface with less danger of laceration, and by a material unirritating and of itself offering but slight obstacles to an immediate union. It seemed probable that all had been accomplished that could be possible, if not all that might be desired, and we need not wonder at the prophecy of Mott concerning his friend, that "in all coming time he would have an enduring monument of his talent, his genius, and his philanthropy, in the gratitude of woman."

"For," as said Dr. Francis, of New York, "prior to the discovery, surgery could do nothing for this formidable class of affections. In Germany, Dieffenbach, Jaeger, Wutzer, and others, had exhausted all their resources in vain. Prolific Germany seems in this instance to have been barren. In France, Desault, Dupuytren, Lallemand, and more recently Jobert, Vidal, and their contemporaries, had been equally unsuccessful, although Jobert claims a success that has never been demonstrated, and I fear that this eminent man, like the late Lisfranc, had scarcely that devotion to practical results, which the written annals of medical science demand from all who give publicity to their cogitations and the issues of their practice. In England, their greatest men, their Coopers, their Abernethys, their Lawrences, their Guthries, could do nothing. Nor have I learned that there has emanated from that practical school of medical and surgical learning which sheds so much glory over Ireland, a single practical idea that can be truly said to have favoured this improvement; and Scotland, while she justly boasts of her Simpson, has yet to be enlightened by that great professor, ere she can add successful results of practice in these cases, to her ample list of chirurgical and obstetrical improvements. In Russia, which proffers claim to our regard for substantial and effective light on several of the obscurest subjects of the healing art, we can testify to no advancement in a knowledge of the intricacies involved in this department of female infirmity. Thus we find universally abroad, nothing but a lame and impotent conclusion to this order of experiments."[1]

There were difficulties, however, attending this operation, which Sims in part foresaw, and with an honourable candor acknowledged to exist. He says:—

"The clamps, burrowing in the vaginal surface, leave a deep sulcus on each

[1] Addresses, &c., at opening of Woman's Hospital, New York, 1856.

side of the new cicatrix, which, when they are removed too soon, fill up by granulation. It is a law of all granulating wounds to contract as they heal, and this contraction on each side of the new cicatrix is often sufficient to pull it gradually apart. Accidents of this sort have happened repeatedly in my hands, from a too early removal of the suture apparatus. Great judgment, which experience alone can give, is necessary to determine the length of time that the sutures ought to remain intact, for no positive rules can be laid down that will answer invariably in every case. I have also seen serious mischief result from leaving the clamps too long imbedded in the parts. Their burrowing and ulceration may extend entirely through the vagino-vesical structure, thereby substituting new fistulous openings for the original one. This complication is by no means incurable, but only prolongs the treatment and postpones ultimate success. In two or three instances I have witnessed a still more serious accident from an undue pressure of the clamps, viz., a strangulation of the inclosed fistulous edges, which unfortunately resulted in a sloughing of the tumefied parts, and a consequent enlarging of the opening."[1]

The experience of the profession for now five years has added to these accidents, others—

"The wires *will* cut themselves out in certain cases, however much attention may be bestowed on their introduction at a sufficient distance from the edge of the raw surface, and sufficient depth into the submucous tissue; the lips included between the clamps will slough, however much judgment may be exercised in drawing them together, and irregularities on the vaginal surface, rigidity from cicatrices, and the situation of either a part or whole of the fistulous opening, may prevent the clamps from being evenly applied, and with sufficient parallelism to secure their regular and efficient action. In consequence of these occurrences the patient has to be subjected to a greater or less number of repetitions of the operation; and, perhaps, other means have to be employed for the perfection of the cure."[2]

Collis, of Dublin, and Spencer Wells, of London, have endeavoured to overcome these disadvantages by methods published during the past winter.

The first consists of Sims' operation with this modification, that both edges of the fistula are deeply split, these fissures separated and secured by India rubber quills, face to face.[3] The second substituted a pin, armed with shot and perforated bars, for the silver wire.[4] These plans, however, had been anticipated by one infinitely better.

It is to Dr. Bozeman, of Alabama, to whom it accidentally suggested itself, that we are indebted for the long looked for discovery, now known as the button suture. His first paper was published in the spring of 1856, and he has lately made known the results of a more extended experience, by diagrams, accurate descriptions, an elaborate classification of all possible varieties of fistula, and directions for perfectly adapting his apparatus to each and every one of them.

The early stages of his operation are identical with those already described. Silver ligatures are used, but are introduced directly and without the aid of any other thread. Instead of being fastened to metal quills, the extremities of the wires, brought together like Mettauer's before twisting, are passed through minute perforations in a shield of lead, which is found to answer much better than the silver at first proposed, and, as by Sims, clamped securely with shot.

It is claimed that the metal shield will—better than Sims' cross-bars—

[1] This Journal, January, 1852, p. 70.
[2] Kollock, work under review, p. 17.
[3] Dublin Quarterly Journal, Feb., 1857, p. 122.
[4] Med. Times and Gazette, Feb., 1857, p. 141.

1. Act the part of a splint in keeping the approximated edges in close contact, and at rest;

2. Prevent the wires from cutting out; and

3. Protect the edges of the wound against irritation by the urine, vaginal discharges, or atmosphere.

The button suture has now been fairly tried. Successful cases have been reported, besides the fifteen or more of Bozeman, in this country, by Gaston, T. Wood, Kollock, Williams, and others; and in Great Britain, by Spencer Wells and Baker Brown; all of whom corroborate its excellence. Kollock's report, indeed, prepared evidently with care and well and candidly written, is mainly occupied by cases from his own practice, showing the relative merits of the clamp and button.

Dr. Bozeman has found that the more difficult cases of ordinary fistula can be easily cured, but there are two lesions he has mastered which have hitherto been entirely beyond surgical aid.

The first of these is a longitudinal laceration of the edge of the meatus, "the most unfavourable form of all the urethral injuries," for which no treatment had ever even been proposed.

The other victory alluded to is in those cases where the cervix uteri is directly involved in the fistula. Nothing had here been done unless by Vidal's method of closing the vagina, save by Jobert; who, until lately, by extensively dissecting away the attachments of the cervix, whether or not accompanying this by the insertion of a flap from a distant part, managed to close the fistula, but lost his patients by peritonitis.

Bozeman, on the other hand, claims better fortune. By his method the uterus itself is dragged down, the edges of the cervix are pared, just as with any part of the vesico-vaginal septum, and stitches inserted into its substance.

The idea of this bold procedure, as novel as it is successful, had undoubtedly presented itself in part to the mind of Velpeau,[1] who speaks of the possibility of dragging down the cervix and making it subservient to closing the fistula, but remarks: "All these suggestions want a foundation to rest upon; none of them can yet adduce any success in their favour;" and Jobert, improving upon himself, relates at the close of the last year a case in which sutures were passed through the cervix;[2] but the credit of having independently conceived the operation in all its completeness, and of having put it into actual practice, is undoubtedly Bozeman's.

The results obtained add only another to the many proofs that the cervix, and, indeed, uterus itself, can bear immensely more manipulation with impunity than is generally supposed.

After-Treatment.—The operation being completed, the great essentials for its success are perfect rest of body (best upon one side) and of mind, a complete stagnation obtainable by opium, of one class of excretions, the intestinal, and a perfectly unimpeded passage of those from the bladder.

In all cases, almost without exception, Sims' catheter, self-retaining by its double curve, is invaluable. When fitting well, of course an essential, it is found to answer every indication. In those obstinate lacerations of the meatus already referred to, this instrument cannot be borne; but, by an ingenious arrangement of his button shield, Bozeman has compelled it, though designed for an entirely different purpose, successfully to take the place by affording the necessary support to a male elastic.

[1] Operative Surgery, vol. i. p. 627.
[2] Union Médicale, Nov., 1856.

The button suture is still in its infancy. It will, undoubtedly, be found useful for other lesions than vesical fistula. We have, indeed, used it already for such, and shall elsewhere report a successful case.

The preceding remarks will have given some idea of "the difficulties," to use Sims' words of his own early experience, "that had to be overcome, the many disappointments that had to be borne before ultimate success; which, as it will be seen, was the work not of a day, and the result not of accident, but of long, laborious, and persevering application."

Almost every variety of vesical fistula in the female seems now to have been brought within the reach and control of art except those invading the cavity of the uterus, and the unique form reported by Simpson as a sequence to pelvic abscess, a fistula between the bladder and rectum without in the least implicating either uterine or vaginal canal.[1] The first class of these cases may yet possibly be made curable after free dilatation of the cervix uteri by carefully placed sponge-tents; but the latter, it would seem, must ever remain beyond hope.

Of other vaginal fistulæ, also the results, most of them, of pelvic abscess, and unconnected in any way with urethra or bladder, we can here say nothing. They form in their relations and treatment a most interesting group among the diseases of women, but their history has yet to be written.

H. R. S.

ART. XIII.—*Adulterations Detected; or Plain Instructions given for the Discovery of Frauds in Food and Medicine.* By ARTHUR HILL HASSALL, M. D. London, 1857.

On the Composition of Food, and how it is Adulterated; with Practical Directions for its Analysis. By W. MARCET, M. D., F. C. S. London, 1856.

THE subject of the adulteration of drugs was first brought before the notice of the American Medical Association at at its annual meeting at Baltimore in 1848. A communication was then read by Dr. T. O. Edwards, at that time member of Congress from Ohio, in which an exposure was made of the great prevalence of adulteration in imported drugs, and of the evils necessarily resulting from it. The Association at that time presented to Congress a memorial on the subject, recommending prompt legislation; and a bill was accordingly passed, the same year, providing for the appointment of drug inspectors in all the principal ports of entry, whose duty it should be to examine critically all medicinal articles imported into the country, and refuse admission to such as were adulterated or in any way deficient in quality. These inspectorships have been continued from that time to the present.

The exposures which were made, at the period referred to, of the worthless character of many imported drugs, attracted immediately the attention of the profession, and will no doubt be fresh in the minds of many of our readers. The discovery that they had been for some years employing in practice opium from which the morphia had been wholly or in part extracted, or scammony which consisted of scammony, flour, gamboge, and chalk, equal parts, was naturally calculated to excite a lively interest in the minds of practical physicians. It was very evident that some check to this wholesale and injurious adulteration

[1] Edinburgh Monthly Journ. of Med. Sciences, Dec., 1852; Obstetric works.

was absolutely necessary; and the appointment of the government inspectors, mentioned above, offered the most direct and efficient means of arresting the evil.

It appears that this measure has been productive of considerable benefit. Six months after the law went into operation, Dr. Edwards made a report to the Secretary of the Treasury, in which he designated the following as the beneficial effects which had resulted from its operation:—

1. An elevation in the quality and purity of the medicinal agents imported.
2. An entire prevention of adulterated and deteriorated drugs from entry and use.
3. No embarrassment to the honest importer and dealer.
4. An increased revenue.
5. Protection to the medical profession and community, an increasing confidence, and an earnest desire on the part of the people for the continuance of the law and its faithful application.

The effect of the law was soon felt, it was said, by the European exporters, so that they ceased to send worthless medicinal articles to this country, as formerly; and consequently a much smaller proportion of the imported drugs were condemned during the second than during the first year after the establishment of the law. Indeed, a committee of the Association were informed by Dr. Bailey, drug inspector for the port of New York, in 1849, that not one-tenth of the spurious and adulterated articles arrived at that time that there did before the passage of the law. The uneasiness of the profession, which had been excited by the unpleasant discovery of the extensive existence of adulteration, was therefore somewhat quieted by the assurance that the evil was effectually arrested, or at least in a fair way to become so.

At the same time, however, a certain degree of suspicion remained behind, that the remedy which had been adopted was not, after all, completely effectual. Dr. Edwards was very possibly right when he claimed, as one consequence of the operation of the law, an entire prevention of adulterated and deteriorated drugs from *entry;* but it is by no means certain that it would as effectually prevent their *use.* If the foreign dealer and manufacturer found it a profitable business to adulterate drugs before importation, there is no reason why our own should not find it equally profitable to adulterate them afterward. The foreign adept in this kind of manufacture need not even allow the business to be taken out of his hands. It would only be requisite for him to establish a "New York Branch" of the original London or Brussels establishment, and he might then continue his operations with the same facility as before. These considerations, accordingly, still weighed with the minds of some of the profession, and prevented their being entirely satisfied with the establishment of the drug inspectorships.

The American Medical Association, furthermore, at their meeting in 1848, appointed a committee of five to report at the next meeting—*first*, the nature and extent of the sophistication and adulteration of drugs, as practised by the wholesale dealers and retail druggists; and, *second*, the best means for the prevention of the evil in its various forms.

Such a report was accordingly made in 1849. The committee state that they have made inquiries of wholesale and retail dealers respecting home frauds, "without obtaining much exact information," the dealers being found "unwilling," from some cause or other, "to give any statements except of a general character;" exciting, in this way, a natural suspicion that home adulterations had already, to a certain extent, taken the place of the foreign. The committee appear to have ascertained, however, the existence of adulteration

in many articles of domestic preparation, as well as in some of foreign manufacture which had passed the custom-house in a pure state; and they come finally to the conclusion that "there are enough" in our country "ready to engage in such dishonest work on a large scale, and so great is the temptation, now that foreign adulterations are excluded from our ports of entry, and the prices of medicines consequently enhanced, that it will require the utmost vigilance of this Association and of the public to prevent their carrying it on."

Notwithstanding this, the committee for 1850, to whom the continued consideration of the subject was intrusted, reported, in general terms, that domestic adulteration seemed at that time not to have increased, but even rather to have diminished; and that adulterated medicines were "not commonly vended in our large cities" (unless by those engaged in the sale of nostrums), except under certain particular circumstances. What these particular circumstances were, did not very clearly appear from the report of the committee. In fact, the special instances mentioned in the report showed so much impurity in the drugs actually in the market, as rather to contradict the conclusions just cited; as, for instance, where samples of rhubarb and cinchona, obtained in Boston, were respectively only one-half and one-eighth the proper strength; and where, of fifteen samples of blue mass, obtained in St. Louis, only one-third gave an approximation to the officinal proportion of mercury. Cod-liver oil, again, was found to be so extensively adulterated that "hardly a tenth," it was believed, of what was sold under that name was genuine, being either refined whale or sea-elephant oil. It seems rather difficult to reconcile such facts as these with the belief that adulterated medicines were "not commonly vended in our large cities."

Still, the report of this committee was, in its general conclusions, rather calculated to quiet the agitation of the subject of adulteration, and to convey the idea that it was, on the whole, not a very gross evil, at least in the Atlantic cities. The committee suggested as remedial measures the following:—

1. That the various State and local medical societies be requested to annually appoint boards of examiners, whose duty it shall be to procure specimens of drugs from the stores within their limits, for examination, and report upon the same to their respective societies at least once in every year.

2. That the respectable druggists and apothecaries throughout the United States be requested to take active measures for suppressing the fabrication and sale of inferior and adulterated drugs; and that it be respectfully suggested to them, wherever practicable, to form themselves into societies or colleges for the promotion of pharmaceutical knowledge and general improvement in their profession.

3. That a committee be appointed, consisting of one member from each State here represented, whose duty it shall be to collect information in regard to spurious and adulterated drugs, and report the same at the next meeting of the Association.

These suggestions were adopted by the Association, and a committee appointed accordingly; but nothing has since been done, so far as we know, in regard to the matter. Practically, the whole work of protecting the community against the use of adulterated medicines has been left, since 1850, to the custom-house inspectors appointed by the law of 1848.

Now, we believe that the almost universal verdict of medical men, even in the large Atlantic cities, at the present time, will be that this law has been *practically ineffectual in preventing the extensive sale and employment of spurious and adulterated drugs.* The complaints of constant disappointment in the operation of simple and important drugs, which ought to be reliable, and

of their extremely variable efficacy, were never more frequent than at the present moment. There can be little doubt that the general skepticism as to the efficacy of therapeutical agents, now so prevalent among physicians, and which has been almost constantly on the increase, is at least partly owing to the deteriorated quality of the drugs themselves. The inefficiency of the custom-house examination depends probably upon two causes. First, the inspectorships are liable, we regret to say, like almost all other government offices in our country, to be regarded as purely political appointments, and to be conferred on purely political grounds, with but little if any regard to the professional qualifications of the appointees. It is to be feared that this mode of conferring appointments has become so interwoven with our political system as to leave but little prospect of its modification for the better, and little security for the capacity, or even the integrity, of the inspectors. Even if this were not the case, however, and if we could be assured that none but genuine drugs were ever allowed to enter our ports, there would still remain a second and much greater difficulty, and one entirely beyond the reach of any custom-house supervision: that is, that drugs, as we have already intimated, are just as liable to be adulterated after as before importation. The same inducements for it are held out to the unprincipled dealer and manufacturer, and the same injurious results to the community follow from its practice.

Dr. Hassall, whose book on the adulteration of food and medicine has more recently called attention to this subject, indicates the only effectual mode of detecting these impositions. Adulterations must be detected in the samples sold or kept for sale by the retail dealer. All other inspections, whether of the crude imported or domestic material, or of the recently manufactured or wholesale article, though useful to a certain extent, must necessarily be incomplete; since adulteration may be, and certainly is, practised, according to the observations of Dr. H., at any point between the custom-house and the counter of the retail druggist. In England an excise law exists, similar in its operation to the inspectorships of the United States; and yet the amount of adulteration practised in the former country, in articles of both food and medicine, is so extensive and scandalous as to excite the indignation of any one who will take the trouble to read over the details which this book presents. *Scammony* made up of "guaiacum and jalap, with woody fibre, cellular tissue, and other insoluble matter;" *powdered jalap*, consisting, for one-third of its bulk, of rasped wood; *ipecac.*, containing "large quantities of carbonate of lime or chalk;" *quinine*, containing gum, starch, chalk, stearine, carbonate of magnesia, &c. &c.; such are the drugs which are actually in the English market, and in daily use by practising physicians in that country.

Now it is a very essential question for us whether we are any better off, in this respect, than the English. We seem to have settled down, since 1850, into a somewhat quiescent state about this matter, trusting to the existing laws for protection. It has already been shown that these laws do not and cannot protect us. Supposing the custom-house requirements to be thoroughly and fairly carried out, the nine years which have elapsed since 1848 have certainly afforded time enough for a tolerably active and quick-witted people to learn all the tricks that were formerly done by foreigners, and to gain sufficient experience for successfully practising them at home. Some further precautions, therefore, must be adopted, unless we are willing to continue in the use of such medicines as are to be found in the market at present. A few reflections, suggested for the most part by Dr. Hassall's book, will show that the subject is fully deserving of all the consideration we may bestow upon it.

Adulteration consists in mixing with the genuine article other substances of inferior value, which increase its bulk and enhance accordingly the profits of the vendor. The foreign substances which are selected for this purpose are usually such as may be readily incorporated with the original article, so as not to be easily detected by ocular inspection. They are, in many cases, simply negative in their properties; and so far, the effect of the adulteration is merely to diminish the efficacy of the medicine; as, for example, where opium is adulterated with clay, or alcohol with water. This adulteration may be carried to such an extent that the properties of the original article are, for all practical purposes, entirely destroyed; and it then amounts to a complete substitution of other materials for those of which it should properly be composed. Hassall states, for instance, that samples are occasionally to be met with, professing to be scammony, which "do not contain a particle of that drug, or small proportions only," and are made up of a variety of ingredients, including even wood and ivory-black.

In most cases, however, the simple adulteration of a drug with an indifferent substance, or its dilution, as it might be called, is not the only or the most important alteration which it is made to suffer. The admixture of large quantities of foreign material necessarily impairs the sensible properties of the drug; and these must be restored or imitated as well as possible, by a further adulteration. Thus Cayenne, according to Dr. Hassall, is extensively adulterated with ground rice, and its colour afterward restored by the addition of red lead, or even the red sulphuret of mercury. Mustard is adulterated first with wheat flour or clay to increase its bulk, then with red pepper to give pungency to the mixture, and lastly with turmeric or chromate of lead to restore its colour. The purchaser is, therefore, not only defrauded of the article which he wishes to procure, but is supplied at the same time with other materials which are absolutely injurious.

It would seem almost superfluous to point out the injurious effects of such practices, and their disreputable character; and yet there is a tendency in the minds of some, resulting principally from a too hasty consideration of the subject, to overlook some kinds of adulteration as unimportant, or to palliate them as excusable. This has sometimes led to a distinction between injurious and harmless adulterations; the former class including those cases in which deleterious substances, such as red lead or arsenite of copper are fraudulently introduced into the mixture; the latter including those in which the bulk of the article is simply increased with some indifferent substance, as where milk is adulterated with water, or calomel with chalk. The slightest consideration will show, however, that *all* adulterations, especially in medicinal articles, are injurious and dangerous, as well as fraudulent. If we prescribe six grains of calomel to a patient who requires the operation of the drug, and three grains of the powder which he takes under that name consist of chalk, it is not merely a pecuniary loss which he suffers, but an actual bodily injury. Even should the adulteration be detected the next day, its effects cannot be counteracted by giving another similar dose of pure calomel; for the time for the most favourable operation of the drug has passed, and the patient's illness is, at the very least, prolonged for twenty-four hours. It is easy to see that much more striking cases than this, and cases quite as likely to happen, might readily be cited. But it is not necessary. There is plainly no proper distinction, except in degree, as to the danger of adulterations in medicine. They are all injurious; and those which are ordinarily the least so, may at any time become extremely dangerous, owing to the accidental circumstances of the case.

But there is another excuse which we frequently hear from those engaged in the trade, and which is too often allowed to pass current, to a certain extent, even among professional men. It is the following: There are, it is said, in all articles of merchandise, different grades of quality, corresponding to the means and taste of the purchasers. Articles of the first quality, which necessarily bear a high price, are within the reach only of the wealthy; inferior goods, at a cheaper rate must be supplied to the poorer classes, for they would otherwise be obliged to go without altogether. Accordingly, there are always to be found in the market goods of these different qualities with corresponding prices. It must necessarily be so; and in the drug trade as well as in others. There is no fraud in this, it is said. On the contrary, it is perfectly well understood that the higher priced articles are always the best, and the cheaper of an inferior quality. If the customer is willing to pay for the best article, he can have it. If he prefers to purchase at a cheap rate, he can be accommodated with an article, corresponding in quality and in price.

Now, it will be observed that the above excuse or explanation, plausible as it seems, rests entirely for its justification on the presumption that the varying quality of the article, corresponding with its price, is *perfectly well known to all parties, purchaser as well as dealer.* So long as this is the case, the variation in quality is certainly not a fraud upon the public, but rather an accommodation. When a man buys a fine broadcloth coat for thirty dollars and a rough peajacket for five, he understands perfectly well the reason for this difference in price. It is evident on mere inspection of the articles; and so long as the articles are good of their kind, and actually are what they profess to be, no harm is done, and the transaction is strictly honourable.

But the case is very different when the inferior article is fraudulently made to resemble the better one, and sold as actually being such. There is a certain class of dealers in clothing, who make a business of getting up, in this way, garments of sham quality; smooth and lustrous externally, but put together of such inferior materials, and in such an inferior manner, that they are ready to fall to pieces after a few weeks' wear. They are sold at a less price than similar garments of the best quality, but they are sold as really being such; and the unsuspecting purchaser is led to believe that he is really obtaining a good article at a lower price than he could get it for elsewhere. This kind of trade is properly regarded everywhere as dishonest; and those carrying it on are not considered as belonging to the class of respectable tradesmen.

Now, the adulteration of drugs, for the purpose of supplying a cheap and more saleable article, is the same kind of transaction with that just described. There is, and can be, in the nature of the case, no difference in the quality of the real drug. Scammony is scammony, and sulphate of quinine is sulphate of quinine; and always of the same composition. But when scammony is mixed with guaiacum and the mixture sold as scammony, or when sulphate of quinine is adulterated with chalk, and the mixture sold as sulphate of quinine, such a transaction is nothing less than the sale of one article in place of another, and is therefore fraudulent and disreputable. It is useless to say that the cheap article cannot be supposed to be as good as the high-priced one, and that the fact of its adulteration is well known to the trade. *The consumer does not know it.* The consumer buys the article, supposing it to be scammony or quinine, and not a mixture of worthless or deleterious substances. In this fact lies the fraud. The truth is, the dealer in adulterated medicines is not guided by any desire to accommodate the public, but simply to enhance his own profits: for though the spurious mixture is

sold at a less price than the pure article, it brings a higher price in proportion than it is really worth. Thus coffee is adulterated with an equal bulk of chiccory, and the mixture sold at a price intermediate between its real value and that of pure coffee. Opium, from which the morphine has been extracted, is sold as a low-priced opium, when it is in reality altogether without value.

The matter, therefore, becomes perfectly simple, as soon as subjected to a moment's examination. The keeping and vending of adulterated drugs are fraudulent, no matter what may be the mode or manner of its performance. It is to be regretted, therefore, that in the report of the Committee on Adulteration, made to the American Medical Association in 1850, some expressions occur, which might be regarded as palliating its practice to a certain extent among the trade.

"Extensive inquiries among physicians, manufacturing chemists and druggists," say the committee, "have led to the following conclusions: *First*, that the wholesale druggists in the large cities, equally in the South and West as in the Eastern States, who are not specially engaged in selling nostrums, either as proprietors or agents, conduct their business on fair and honourable principles. As a general rule, they buy their choice chemicals from those who manufacture them, and either import other articles, or get them directly from those who do; and are always disposed to supply good articles to customers who are willing to pay a remunerating price. At the same time, many of this class keep inferior articles which they dispose of for a corresponding price to physicians and storekeepers who insist on buying at reduced rates."

Now, it is difficult to reconcile this trade in "inferior," that is adulterated, drugs with "fair and honourable principles" of business. These adulterated drugs are purchased by the retail dealers in order to be sold as genuine. It is not true that the public are ever parties to the transaction; or that they prefer a cheap article, knowing it to be adulterated. No man, suffering with intermittent fever, would buy a cheap quinine in preference to a high-priced one, if he knew that he would be obliged to take a teaspoonful of the former for every grain of the latter. The retail sale, which is the end and object of all previous trade-sales, is always a fraud. The wholesale dealer knows this perfectly well, and is, therefore, a party to the transaction, when he deals in articles which he knows to be destined for that purpose. Keeping and selling medicinal substances, therefore, knowing them to be adulterated, under the pretence of supplying customers who wish a cheap article, is but little, if any, less injurious and disreputable than actually making the adulteration or retailing the spurious drugs.

We have been led to make the foregoing remarks because it seemed to us that the profession in this country had been lulled into a kind of false security with regard to this subject, and to the amount of protection afforded them by the existing laws.

Dr. Hassall's book possesses a still more general interest from the fact that it treats extensively of the adulterations in food as well as of those in medicine. Indeed, the greater part of the book is occupied by the former topic. The author shows that in England at the present day the most important articles of food and drink are hardly less adulterated than medicines. Some of these adulterations are practised abroad, some of them at home; some on foreign and some on domestic articles. In some cases the genuine and spurious materials are both imported from abroad, and mixed after being brought into the country. Dr. H.'s statements have the greater value since they are not merely the result of general inquiries among manufacturers and dealers,

but of direct examination of samples purchased at retail, and consequently in the same condition as they are ordinarily obtained by the consumer.

An extremely important feature of the work is the extensive application of the *microscope* to the detection of foreign matters in alimentary or medicinal substances. Many adulterations have heretofore escaped detection in consequence of the inability of the chemist to recognize them by any means at his command. Some adulterations are so coarse that they may be recognized, by any one familiar with the appearance of the genuine article, by a careful ocular inspection; as where foreign leaves are mixed with those of tea or senna. In other instances, where inspection would fail, a chemical examination is sufficient; as where calomel is adulterated with substances which are not volatilized by heat, or which are soluble in water. There are other cases, however, principally those of powdered vegetable or animal substances, in which ocular inspection and chemistry are equally at fault; as ground coffee, for instance, adulterated with chiccory or exhausted tan, or mustard adulterated with wheat flour and turmeric. Here, however, the microscope steps in and accomplishes all that could be desired; for no amount of grinding and powdering can destroy the shape of the vegetable cells and fibres, or the optical characters of starch-granules peculiar to different kinds of vegetable substances. Thus the minute anatomical structure of all the different kinds of flour is readily recognized when these are mingled together. Chiccory is detected in coffee, potato flour in arrowroot, and the fibres of rasped wood in powdered opium. We know of no application of the microscope, yet made, which has been more directly and practically serviceable than this.

Tea is adulterated, according to Dr. H.'s investigations, with various foreign leaves, such as those of the beech, elm, horsechestnut, plane, willow, poplar, hawthorn, and sloe. Two or three kinds of foreign leaves are mixed with the tea by the Chinese themselves, previous to exportation. The Chinese manufacture also a spurious article for the purpose of admixture with genuine varieties, which they designate by the expressive name of "lie tea." It consists of the dust of tea-leaves, sometimes of foreign leaves, and sand, made up by means of starch or gum into little masses, which are afterwards painted and colored so as to resemble either black or green gunpowder. "This article," says Hassall, "although the chests containing it are branded with the words 'lie tea,' was at a recent period extensively imported into this country, and of course found purchasers."

But it is in the colouring and dressing of the real tea-leaves that the most important, because the most extensive and deleterious, adulteration is practised. Tea drinkers will probably be surprised to learn that with every cup of green tea they swallow so much *mineral paint*, artificially put on in order to increase the brilliancy and lustre of the leaves.

"It is with green tea," says Dr. H., "that the practice of artificially colouring the leaves is carried to the greatest extent. The varieties of green tea imported into this country from China are Twankay, Hyson-skin, Young Hyson, Hyson, Imperial, and Gunpowder. Now the colour of the whole of these teas, without a single exception, is artificial, and caused by the adhesion to the leaves of various colouring matters.

"The usual colouring matters employed are ferrocyanide of iron or Prussian blue, turmeric, and China clay. These are mixed in various proportions, so as to produce different shades of blue and green; the surface of the leaves being moistened, they are then agitated with the mixtures until they become faced or glazed, as it is termed. Occasionally other substances are employed by the Chinese, as indigo and sulphate of lime, or gypsum. In proof that it has long been the practice frequently to colour green tea artificially, we have the evi-

dence of various travellers; but the most conclusive and complete evidence, both as to the extent of the practice and the nature of the ingredients used, has been supplied by the microscope."

In these cases, then, the consumer gets green tea, and various colouring matters in addition. But in other instances he gets the colouring matters alone. One branch of the adulterating business in England consists in buying up tea-leaves which *have been already used and exhausted*, drying them, colouring them artificially, adding sulphate of iron or catechu to restore the astringency, and reselling them as black or green tea. The colouring matters employed for this purpose Dr. H. found to be frequently more injurious than those used by the Chinese; viz., rose pink, Dutch pink, chromate of lead, Venetian red, soapstone or French chalk, carbonate of lime, carbonate of magnesia, carbonate of copper, *arsenite of copper*, Prussian blue, and indigo.

Coffee is adulterated with chiccory, roasted flour, scorched peas and beans, roasted carrots, mangel-wurzel, acorns, mahogany sawdust, burnt sugar, Venetian red, and *baked livers*. This last article is one so little likely to be suspected beforehand, that we give a short description of the process, quoted by our author from a work on coffee, published four or five years since.

"In various parts of the metropolis, but more especially in the east, are to be found liver bakers. These men take the livers of oxen and horses, bake them, and grind them into a powder, which they sell to the low-priced coffee-shop keepers, at from 4*d*. to 6*d*. a pound, horse's liver coffee bearing the highest price. It may be known by allowing the coffee to stand until cold, when a thick pellicle or skin will be found upon the top. It goes further than coffee, and is generally mixed with coffee, and other vegetable imitations of coffee."

Why baked livers should be especially selected for this purpose does not at first sight appear. It is evident enough, however, as soon as we have become a little familiar with the "fundamental principles" of adulteration. Horses' and bullocks' livers are, in the first place, cheap. Secondly, the biliary matters with which they are imbibed serve to imitate tolerably well the colour and bitterness of real coffee. They are therefore much better adapted for this purpose than other internal organs, such as the spleen, kidneys, or brains. Coffee, again, which has been largely adulterated with scorched flour or beans, has its colour and bitterness partly restored by the addition of burnt sugar.

We subjoin, as a curiosity in its way, the following, from page 119:—

Results of the microscopic examination of thirty-four different Coffees, of all qualities and prices, and sold under the following attractive titles:—

COFFEES OF HIGH PRICE.

1. *Finest Mocha Coffee.* No adulteration.
2. *Noted Old Mocha.* No adulteration.
3. *Finest Jamaica Coffee.* No adulteration.
4. *Rich Old Mocha.* Of chiccory, a good deal.
5. *Best Old Mocha.* A little chiccory.
6. *Fine Old Turkey Coffee.* Much chiccory.
7. *Very Fine Mocha.* Much chiccory.
8. *Genuine Old Mocha.* A little chiccory.
9. *Finest Turkey Coffee.* Contains chiccory.
10. *Celebrated Old Mocha.* A good deal of chiccory.

COFFEES OF MEDIUM PRICE.

11. *Costa Rica Coffee.* Nearly one-half chiccory.
12. *Fine Jamaica Coffee.* Contains a considerable quantity of roasted corn.
13. *Delicious Coffee.* Roasted beans and chiccory, forming about one-third of the article.

14. *Plantation Coffee.* Of roasted corn much, with some chiccory, both not less than one-third.
15. *Finest Turkey Coffee.* Much chiccory, and some roasted corn; very little coffee.
16. *Celebrated Jamaica.* Very little coffee; principally chiccory.
17. *Finest Berbice Coffee.* About one-half coffee, much chiccory, and some wheat.
18. *Splendid Turkey Coffee.* About one-half coffee, the rest chiccory.
19. *Fine Plantation Coffee.* One-third coffee, the rest chiccory, with a little roasted corn.
20. *Beautiful Jamaica Coffee.* Two-thirds coffee, the rest chiccory, with a little corn.
21. *Finest Java Coffee.* Half coffee, much roasted corn, with a little chiccory.
22. *Superior Plantation Coffee.* Three-fourths coffee, the remaining chiccory.

COFFEES OF LOW PRICE.

23. *Fine Mountain Coffee.* Four-fifths coffee, one-fifth chiccory.
24. *Parisian Coffee.* Principally chiccory and corn; very little coffee.
25. *Superb Coffee.* The principal part corn and chiccory; very little coffee.
26. *Rich Drinking Coffee.* One-third coffee, the rest chiccory, with some roasted corn.
27. *Very Excellent Coffee.* One-half coffee, the other mostly chiccory.
28. *Delicious Family Coffee.* One-fourth coffee, three-fourths chiccory.
29. *Fine Ceylon Coffee.* Very little coffee, a great deal of chiccory, some roasted corn.
30. *Fine Java Coffee.* Much chiccory and some roasted potato; very little coffee.
31. *Coffee as in France.* Principally chiccory.
32. *Very Excellent Coffee.* Principally chiccory.
33. *Fine Plantation Coffee.* Nearly all chiccory; very little coffee.
34. *Delicious Drinking Coffee.* A large quantity of chiccory, and much roasted corn.

Cocoa, sugar, honey, milk, flour, butter, lard, arrowroot, and their adulterations, are all described in a similar manner. With many new and unsuspected adulterations, discovered by Dr. Hassall, there are some, popularly supposed to be very common, which he shows to be either quite rare, or even not to have an existence. Thus sugar is generally thought to be extensively adulterated with sand; but Dr. H. found no sand in over one hundred samples of sugar which he subjected to examination. The impurities which he met with were starch, treacle, glucose, fragments of sugar-cane, fungous sporules, and specimens of the acarus sacchari. Milk, again, very seldom contains chalk, contrary to the general belief. Dr. H did not meet with it in a single instance. The most prevalent and important adulteration of milk is with water; after which the operator adds molasses or syrup to sweeten it, salt to give it a flavour, and anatto to colour it. Starch and sheep's brains are sometimes added, in order to restore the opacity to diluted milk; but these adulterations are rare, the dealers not usually taking the trouble to practise them. These impurities would furthermore, like chalk, be at once detected, on allowing the milk to stand, by subsiding to the bottom of the vessel as a visible deposit.

A very amusing chapter is that on what the author calls "proprietary alimentary preparations;" that is, various mixtures which are prepared of cheap ingredients, patented, and then sold at a dear rate under some high-sounding title; such as Revalenta Arabica, Nutritious Farina, Semola, Semolina, and the like. These substances consist mostly of baked flour, or even of cheaper ingredients, sometimes scented and coloured, and sold at prices varying from twenty-five to sixty-eight cents per pound; their real value, according to their

composition, not exceeding in any case five or six cents per pound. Thus the author gives analyses and microscopic drawings of the following of these articles among others:—

Dubarry's Revalenta Arabica; consisting of starch-granules of the Arabian lentil, barley flour, sugar, and salt.

Wharton's Ervalenta; consisting of a mixture of the French or German lentil with a substance resembling maize or Indian corn meal.

Gardiner's Alimentary Preparation; consisting of very finely ground rice.

Leath's Alimentary Farina, or *Homœopathic Farinaceous Food;* consisting principally of wheat flour, slightly baked, sweetened with sugar, together with potato starch, Indian corn meal, and tapioca.

Bullock's Semola; consisting of the gluten of wheat, with a proportion of wheat starch.

Maidman's Nutritious Farina; consisting entirely of potato flour, artificially coloured of a pink or rosy hue.

Plumbe's Improved Farinaceous Food; composed of bean or pea flour, some potato flour and a little arrowroot.

Palmer's Vitaroborant; consisting of a mixture, sweetened with sugar, of wheat flour with the red or Arabian lentil.

The flour, sugar, &c., were introduced, in many of these preparations, merely to diminish the strong flavour of the lentils, which is sometimes disagreeable.

"Extremes meet," says the author; "lentils, being somewhat cheaper than peas, are supplied to many of our workhouses, to be used in the preparation of soup, &c. Thus they are not only consumed by paupers, but by the rich, the chief difference being that the latter frequently pay 2*s.* 9*d.* per pound for them."

Dr. H. suggests also one or two receipts for preparing similar mixtures, which shall have all the advantages of the patented articles, if any such there be, without their exorbitant price.

"As the cost of most of the prepared lentil powders sold as ervalenta, revalenta, &c.—viz., 2*s.* 9*d.* per pound—forms a very serious obstacle to their use, supposing that it is in any respect desirable that they should be more generally consumed, we have framed the two following receipts, whereby a considerable saving of expense may be effected:—

1st Receipt.

Red or Arabian lentil flour	2 lbs.
Barley flour	1 lb.
Salt	3 oz.

Mix into a uniform powder.

"The red lentil may be obtained of almost every corn chandler at about 4*d.* per quart; the cost of *our* ervalenta would be about 2*d.* per pound; and it is perfectly clear from the analyses which we have given above, that whatever may be the advantages possessed by the much vaunted ervalentas, revalentas, &c., that our article must contain them all."

2d Receipt.

Pea flour	2 lbs.
Indian corn flour	1 lb.
Salt	3 oz.

Mix as before.

Increase of bulk is not the only object for which adulterations are practised. We have already seen that, in the case of teas, foreign and sometimes

poisonous substances are added merely for the sake of improving the colour and external appearance of the article. This is still more remarkable in the case of *pickles*. These articles of food are almost universally more or less artificially coloured, and generally with some preparation of copper. This adulteration is sometimes so excessive as to be readily detected, even by the eye. Every one must have noticed samples of pickles kept for sale by the grocer, in which the green colour was unnaturally strong; the preserved pickles being often, in fact, greener than the same vegetables when in a fresh condition. This green colour has even sometimes a distinct and altogether unnatural shade of blue. Now, in all these instances, the colour depends upon the presence of some salt of copper; either the sulphate (blue stone) artificially added, or the acetate produced by the action of the vinegar on metallic copper. When metallic copper is used, the sulphate is frequently formed as well as the acetate, owing to the previous adulteration of the vinegar with sulphuric acid.

The examination of twenty-three samples of pickled vegetables led Dr. H. to the following conclusions:—

1. That the vinegar used for pickling is of a very weak description, the percentage of acetic acid ranging between 1.48 and 2.91. It will be remembered that vinegar of good quality ought to contain from four to five per cent. of pure acetic acid.

2. That nineteen out of twenty of the vinegars submitted to analysis, poor as they were, yet owed a portion of their acidity to sulphuric acid, the amount of which varied in the different samples from .38 to 2.52 in the 1000 grains; the largest quantity of this acid being detected in the vinegars in which the red cabbages were pickled.

3. That in the whole of the sixteen different pickles analyzed for copper, that poisonous metal was discovered in various amounts; two of the samples contained a small quantity; eight, rather much; one, a considerable quantity; three, a very considerable quantity; in one, copper was present, in highly deleterious amount; and in two, in poisonous amounts.

4. That the pickles which contained the largest quantity of copper were those which consisted entirely of green vegetables, as ghirkins and beans.

The author presents a most repulsive picture of the composition of *potted meats and fish*, as subjected to his examination. These articles are but little used in this country, but in England they are extensively employed and are generally regarded as a delicacy. The form of a homogeneous paste is, however, that which presents the greatest facility for adulteration; and these preparations are accordingly found to contain not only such ingredients as flour and starch, and to be partly made up of inferior qualities of meat and fish, but to be artificially coloured also, in most instances, with Venetian red or Armenian bole. These earthy substances are added, according to Dr. H., not only for the purpose of heightening the colour of the mixture, but also to conceal the dirt contained in the brine, in which the fish is imported.

The artificial colouring of *sugar confectionary* is much worse than the above. These articles are sometimes coloured all over with the same tint; and are sometimes parti-coloured, two or three different tints being applied, for the sake of ornament, to different parts of the same piece. From the examination of 141 samples, Dr. Hassall arrived at the following result:—

Fifty-nine were coloured with CHROMATE OF LEAD.

Eleven with GAMBOGE.

Twelve with RED OXIDE OF LEAD.

Six with BISULPHURET OF MERCURY (vermilion).

Eight with BROWN FERRUGINOUS EARTHS, Vandyke brown, umber, or Sienna.

One with INDIGO.

Twenty-four with PRUSSIAN BLUE.

Ten with a mixture of CHROMATE OF LEAD and PRUSSIAN BLUE, making several varieties of green.

One with CARBONATE OF COPPER.

Nine with ARSENITE OF COPPER.

Four with CARBONATE OF LEAD.

Enough has been said to show the great extent of the above adulterations, and the abominable consequences that are liable to result from them. It must be recollected that many of these substances, fraudulently introduced into food, are actually poisonous; and, furthermore, that some of them belong to the class known as cumulative poisons. The small quantities in which these substances are introduced, day by day, is therefore no protection against their finally producing poisonous effects. Whoever takes Cayenne pepper every day upon his salad, is liable to be dosing himself at the same time with red oxide of lead; and the lover of green tea may after a time find his eyelids swelling and his legs aching from the arsenite of copper with which the leaves were coloured. Lest it should be supposed that such dangers as these are altogether imaginary, we subjoin the following account of a case in which lead palsy was produced by taking snuff; an article which is not unfrequently coloured with chromate of lead, or the red oxide of the same metal. The case is given on no less an authority than that of Mr. Erichsen; and it is of so remarkable and interesting a character, that we extract it entire. It is from page 617 of Dr. Hassall's book:—

"*Case of slow poisoning by Snuff containing Lead.* By Mr. ERICHSEN.—Whilst on a professional visit in the country, last March, I was requested to see a gentleman who had been invited down to a friend's country-seat, in the hopes that change of scene and air would influence favourably an attack of paralysis, which was said to be of a rheumatic character, by which he had been disabled from work for many months past, and of which he despaired of recovering, having relinquished all treatment.

"I found the patient in bed, and somewhat exhausted by the journey down, a distance of nearly a hundred miles from his usual residence. He was peculiarly sallow, the complexion having almost an icteric tinge; but the countenance was lively and expressive, and the intellect as bright as usual.

"Mr. A. B. could stand, and, if supported, could walk, though feebly and with much difficulty. He complained much of pain about the shoulders and the fleshy parts of the thighs and legs, and especially of burning sensations in the soles of his feet. The articulations all appeared healthy; no swelling or looseness was perceptible about any of them.

"I was, however, particularly struck with the appearance of the hands and arms, which were lying powerless on the coverlid of the bed. There was marked 'wrist-drop' of both arms, the hands hanging flaccid and at right angles with the forearms, without the patient being able to extend or raise them in the slightest degree. There was, however, some slight power of extension left in the fingers, especially in those of the left hand. Though unable to extend the fingers, raise the hand, and scarcely having power to elevate the arm, Mr. B. could *flex* the fingers pretty firmly, so as to give a tolerably good grasp to whatever was put into his hand. The index finger of the right hand seemed to be the most affected, and was permanently flexed.

"There was a very marked degree of wasting of the whole mass of the extensor muscles of the forearm, so that a longitudinal hollow corresponding to the interosseous space was perceptible down the whole length of the forearm, and a very deep and marked depression in the interspace between the first and

second metacarpal bones. The hands were quite powerless, and the patient was unable to render himself the slightest assistance.

"The tongue was pale and flabby; and on examining the gums, I found a deep blue-black or leaden-coloured line around the teeth, more marked about the molars. Digestion was much impaired. Appetite capricious, with much flatulence, and occasional attacks of constipation, with colicky pains.

"On inquiring into the history of the case, I learnt that Mr. A. B., who is much devoted to literary pursuits, and habitually led a sedentary life, had for some years previously suffered from pains of a rheumatic or gouty character; that in May, 1853, he had been attacked by constipation and colic while lodging for a short time in a newly-painted house. In August of the same year he had first begun to lose power in extending his arms, finding a difficulty in raising them to put on his coat; and from this time the paralytic symptoms gradually increased, until they had assumed the degree in which I found them, when he had become reduced to a state of complete physical helplessness; though, as I have already observed, his powerful and clear intellect was as perfect as ever.

"On examining Mr. A. B., I was at once struck by the very marked 'wrist-drop,' more complete than I had ever seen before; the limitation of the paralysis to the extensors, which were greatly wasted; the existence of a blue line around the teeth; and the occurrence of occasional attacks of constipation and colic, together with flying pains in the fleshy parts of the body, with absence of all articular inflammation. These symptoms led me to the conclusion that Mr. A. B. was suffering from saturnine paralysis, and that he had been slowly poisoned by lead.

"The difficulty was, however, to ascertain how poisoning by lead could have been effected. With this view, I made diligent inquiry into the patient's habits, the water he drank, the utensils he used, &c., but could not detect any source to which the presence of the mineral in the system could be traced, except that the first attack of colic and constipation had occurred whilst temporarily lodging in a house which smelt of fresh paint; but as he soon left this, I thought it very insufficient to explain his continued and increasing sufferings. In the course of my inquiries, however, I found that he took snuff in considerable quantities; I accordingly emptied his box of its contents, and took them up to town with me with a view to further examination. The snuff was analyzed by Professor Williamson, who detected in it a considerable quantity of lead; and another supply having been procured from the shop at which Mr. A. B. was in the habit of purchasing it, was subjected to analysis by Dr. Garrod, who readily detected large quantities of the metal in it.

"Mr. A. B. was now put under treatment for saturnine paralysis. The snuff was left off; the bowels were kept open with the acidulated sulphate of magnesia; iodide of potassium was freely given in conjunction with strychnia, which was applied topically to blistered surfaces as well as administered by the hands; and galvanism was assiduously employed. Under this plan of treatment he gradually improved in all respects; the colicky symptoms rapidly disappeared, the muscular pains subsided, and the paralytic condition of the extensors was gradually removed, until at the end of July he was able to resume and to discharge public duties of a very onerous character with his usual ability and energy.

"With the above sketch we received from Mr. Erichsen a sample of the snuff which was the occasion of all the mischief. On analysis it was found to contain 1.2 per cent. of red oxide of lead; that is very much less than some of the other samples, the analyses of which have already been given."

The author goes through, in a similar manner, with the description of vinegar, spices, cheese, ale, porter, spirits, wines, &c., and their adulterations. We will not, however, dwell further on the very interesting details which he presents, but refer the reader for them to the book itself.

In conclusion, we would offer some remarks on the adulteration of food and medicine in our own country, and the means of protecting ourselves from it, for which we believe the reader is already prepared. It is plain that

the adulteration of food is a serious injury, not to the purse only, but to the health of the consumer. That of medicine is certainly not less deleterious. It is always fraudulent and may at any time become even homicidal in its consequences. By its operation all the devotion, skill, and judgment of the practical physician are set at naught. A patient has perhaps swallowed, intentionally or by accident, an overdose of laudanum. The physician who is called finds him already half comatose; and he knows that ipecac and tartarized antimony are too slow in their operation to be trusted as emetics. Sulphate of zinc is not at hand; and if it were, half of it would, perhaps, consist of Epsom salts. But there is mustard; nearly always to be found ready in every family, the most active, prompt, and local in its operation of all emetics, and the most stimulating to a semi-narcotized stomach. He mixes a liberal dose, succeeds by dint of perseverance in compelling the patient to swallow it, and awaits the result. But, unfortunately, the mustard was partly clay, partly plaster of Paris, and partly turmeric, with a little real mustard and red pepper to give it pungency. No vomiting follows. Another dose is forced down, with greater difficulty than before, with a similar ineffectual result; and by the time the assistant arrives with a stomach-pump, the patient's blood is loaded with the narcotic, and he is fairly beyond the reach of help from either medicine or surgery.

Take another instance. A physician wishes to give an emetic to a slender and delicate child, who has eaten the wrong thing at dinner and is sick in consequence. Sulphate of zinc or copper would be evidently unnecessary and inappropriate. Tartarized antimony especially is to be avoided, on account of its depressing effects and the persistence of its operation. Ipecac, mild and effectual as an emetic, without any violent depressing constitutional effects—is the drug which he judiciously selects. But his judgment is without avail; for the ten grains of ipecac administered to the little patient contain eight grains of liquorice and one of tartarized antimony.

Now, let it be remembered that such cases as these are liable to occur at any moment in the practice of any physician. Similar instances, where the immediate effects are not quite so disastrous, evidently do occur daily, and do not require to be especially designated. In view, therefore, of the moral character, the intention, and the consequences of these adulterations, there can be but one conviction as to the necessity of legislative interference, and the manner in which it should be exercised. *The adulteration of food or medicine should be made a felony; and should be placed upon the same level with the coining of false money, and the counterfeiting of bank-notes.* The dealer who vends a spurious article, knowing its character, would then be placed in the position of one who passes a counterfeit bill, knowing it to be counterfeit. The consumer would then have every reasonable protection. The honest trader would not be compelled, in self-defence, to adopt the practices of the unscrupulous, or at least to wink at their existence, as he does at present; and, finally, the practical operation of medicines would no longer disappoint the physician and discourage the patient, as they too often do at the present day. It is to be hoped that both the National Association and the local societies will continue to agitate the question, until they succeed in bringing it properly under the notice of the State legislatures.

J. C. D.

ART. XIV.—*Of Nature and Art in the Cure of Disease.* By SIR JOHN FORBES, M. D., and C. L. OXON, F. R. S., Fellow of the Royal College of Physicians, Physician to the Queen's Household, &c. &c.: London, 1857, pp. 264.

THIS book requires at our hands something more than a brief bibliographical notice; for it contains the fullest and most satisfactory development that we have yet seen of a subject, which has lately attracted extensive and deep attention from thinking minds in our profession. We do not mean to say that it is a perfect book. It has many faults and short-comings, as the reader will see as we proceed with our review of it. But it is, nevertheless, so good a book—so full of wise instruction on many of the most important points in therapeutics, and so clear and forcible in its exposure of many very prevalent errors in medical practice—that we hope it will be reprinted in this country, and have a large circulation in the profession, and also among intelligent men in the community at large.

Although the subject of this book is more extensively and systematically examined here than it has been anywhere else, the discussion of it is very far from being exhausted. Indeed, it is absolutely necessary that the investigation should be pushed far beyond those points which Sir John Forbes brings out, in order that the whole duty of the profession may be compassed, and its therapeutics be placed upon its right basis. Medical practice is everywhere still in an exceedingly unsettled state; and there will obviously be needed much more and closer investigation of the relations of art and nature in the treatment of disease, to establish correct views of them in all their complications. Such labours as those of Forbes in this volume do, it is true, settle some points; but these are few in comparison with the many that yet remain to be settled. As we read we are prompted continually to look forward to the results hereafter to be realized from the observation of disease, carried on under the influence of the suggestions presented here and in other books and papers on similar subjects. Great changes in medical practice have taken place in the last twenty or thirty years from such influences, and we anticipate greater still in years to come. The indications of this are not to be mistaken. Practical medicine is in one of its great transition periods, and a more eventful one than has ever before transpired. The glory of the future of our art is, it is true, seen but dimly; and yet it is seen sufficiently by the intelligent and earnest physician to awaken him to the most strenuous efforts for its advancement. That these anticipations are not unfounded we think will appear as we go on with the examination of the work before us.

Our object in this article is threefold—to aid in diffusing the spirit of the book and its prominent ideas in the profession; to point out some of the errors of the author; and to induce others to engage in the investigation of the subject. In regard to this latter point, as already hinted, it is plain that we are as yet at the mere threshold of all that is to be discovered and settled. Although much has been written on the subject, both formally and incidentally, in book and in pamphlet, especially of late years, there is much more to be written; for it is in the investigation indicated that therapeutics is to find its chief means of advancement. It is to the discovery of the capabilities of nature and of the proper limitations of art in the cure of disease that the attention of physicians at the present time should be principally directed.

In the introductory chapter, Sir John Forbes speaks of his book in the following very appropriate language:—

"Having been now actively engaged in the practice of Medicine for the long period of fifty years, and having derived therefrom much of the prosperity and happiness that have been my fortunate lot in life, I feel that my profession has claims on me for much more than I have hitherto been able to give it; and as, at my time of life, and in my present state of health, I have no right to look forward to the acquisition of further knowledge in the same field, it is incumbent on me to communicate now any information I may possess, if I am to communicate it at all. In doing so, I cannot help being impressed with the feeling of solemnity which naturally accompanies any act that is to be the last of its kind. And, in this mood, I would fain regard the present work in the light of A LEGACY TO MY YOUNGER BRETHREN, which, slight as it is, may not be found altogether unworthy of their acceptance. I would, indeed, bequeath it in full confidence of its value, if I might reckon on its being received in the same way as the legacy of the Pot of Gold, in the fable, was received by the rustic testator's sons. If my book—though, like the old man's vineyard, really containing in itself no gold—should only lead to the zealous cultivation of the subject of which it treats, the result could not fail to be of inestimable value to the cultivators. For, on the profounder, more critical, and purer study of Nature, as manifested in disease, rest, in my judgment, the best hopes of improvement in the medical art; and to this study the spirit of my book may, at least, lead the way and give the initiative, if its actual contents are found of lesser importance."

We wish that the profession had more of such legacies. Though we would be far from encouraging any great number of physicians to give to the public in their old age the lessons of experience which they have gathered, from a fear of a predominance of garrulity in our medical literature, we wish that we had more of such works as this of Sir John Forbes, the Medical Notes and Reflections of Sir Henry Holland, and the Letters to a Young Physician, by Dr. James Jackson, of this country.

Our author has not written this book for the profession alone—he wishes to enlighten intelligent and thinking men in the public at large.

In relation to this our author very justly remarks:—

"It cannot be doubted that juster views of the nature of medical science and of medical art, if once prevalent among the lay public who are well informed, will, like all other knowledge, descend to those who are not so; and thus the progress of rational medicine will be facilitated, and the hands of those professors strengthened, who have the courage to advocate and practise their art conscientiously, however opposed to vulgar prepossessions and prejudices."

The practicability of any such effort to enlighten the public is doubted by some physicians. They claim that the subject of practical medicine is of so difficult and complex a character, that no one can understand it who does not make it the study of his life; that, therefore, all attempts to explain its principles to men out of the profession will prove futile; and that the public must learn to take physicians wholly upon trust, and not inquire into their practice. But this is going altogether too far. While we would have non-medical men appreciate most fully the difficulty of understanding therapeutics, and deny them the right to sit in judgment on the practice of the physician simply because they are incompetent to do so; we believe that there is a common ground, and one of no narrow limits, on which physician and patient can meet and act together in the cure of disease. And we believe that this common ground will be constantly widening, as medical practice shall be stripped of its needless mystery, and be rendered more consonant with nature

and plain common sense, and as thinking men in all classes of the community shall become properly informed through the efforts of physicians, who, like our author, are willing to take the pains to teach them what it is possible for them to understand on the subject. There is much, it is true, in medicine that is necessarily mysterious to the practitioner, and much more that is so to the non-professional observer; but this furnishes no reason for shutting out the latter any more than the former from learning what he is able to comprehend.

The time for implicit blind trust in the physician has gone by, and we are glad that it is so. Be it that there are numerous errors floating in the public mind, arising out of its disposition to opinionate on medical subjects. We had rather have this than stagnation, for it is a state of transition leading to something better. This mental activity on the part of the public in relation to medicine is not to be forever breeding error. It can be enlightened, and thus be brought into the service of the physician. But so long as it is not enlightened, it will be retained in the service of the quack and the quackish practitioner, and will thus continue to prove a serious obstacle to the advancement of our profession. The duty of medical men then is clear. If they desire to promote the interests of medical science, they must enlighten the public, so far as it can be done, in relation to the principles on which they act in the treatment of the sick. In truth, this has already been done to a considerable extent both by public and private efforts. A great amount of information on medical subjects has been scattered by physicians among the people, especially during the past twenty or twenty-five years. To say nothing of other publications, the exposures which have been made of homœopathy and other forms of quackery, have taught intelligent men in the community much in regard to the principles of medicine. And then in the private intercourse of physicians with their patients and friends a vast deal has been done in the removal of error and the diffusion of truth.

It would take us into too wide a field if we should notice all the evidence that we have that this enlightenment of the public mind has been thus in progress. We will allude to but one fact—one which every intelligent physician must observe in his daily intercourse with his patients. It is the fact, that the number of those with whom he can deal plainly and without the disagreeable necessity of a resort to placeboes, whenever he deems it proper to give but little or no medicine, is continually increasing. This would not be so if there were not a gradual diffusion of the principles of rational medicine in the minds of the people.

We would have these efforts, both private and public, for popular enlightenment in regard to medicine still plied by physicians as opportunities offer for doing so. Our chief business, it is true, is the improvement of our art; but at the same time we can, with profit alike to ourselves and the community, diffuse rational views of medicine among the people, by efforts of an incidental character. Especially, can this be done in our daily intercourse with patients. It is true in this as it is in everything else, that more is accomplished for the general good by the aggregate of little, or rather limited influences, exerted by the many, than by the greater and more public efforts put forth by the few.

We have been led into more extended remarks on this point than we intended to make; but the subject is so important that we should be glad to dwell upon it longer if our limits would permit.

Our author, in speaking of the object of his work, says that it

"Is not simply to exhibit, in an independent manner, the general fact of the respective and relative powers of Nature and Art in curing diseases, but to establish the more special fact that Nature possesses vastly greater powers than Art in curing diseases, and, consequently, that its extent is beyond the common belief of the junior classes of medical men, and men in general."

In proving this general proposition, he treats in separate chapters of many subordinate topics such as these: the ignorance existing respecting the power of nature to cure diseases; general notions of diseases; the abuses, mode of production, and nature of diseases; the cause and progress of diseases; the natural terminations of diseases, and the modes in which they take place; evidence in favour of the curability of diseases by nature; the existence and general nature of the medical art; instruments of the medical art; the direct and specific action of some of these instruments, and the indirect and vicarious action of others. His concluding chapter contains a general estimate of the powers of the medical art.

Here we have a wide range of topics, and all of them are treated very fully, the author bringing out freely the results of his experience, covering the space of fifty years. We think that the best chapter in the book is that which treats of "the evidence in favour of the curability of diseases by nature." The evidence on this point is brought out very copiously and clearly under eight different heads. The conclusion at which he arrives is thus expressed:—

"The one great result obtained from the study of these various authorities is this: that the power of nature to cure diseases is infinitely greater than is generally believed by the great body of medical practitioners, and by the public generally. So great, indeed, is this power, and so universally operative, that it is a simple statement of the facts to say, that of all diseases that are curable and cured, the vast majority are cured by Nature independently of Art; and of the number of diseases that, according to our present mode of viewing things, may be fairly said to be cured by Art, the far larger portion may be justly set down as cured by Nature and Art conjointly. The number of diseases cured entirely by Art (of course I omit, in all these statements, *surgical* art), and in spite of Nature—in other words, the number of cases that recover, and would have died had Art not interfered, is extremely small."

Some may be disposed to doubt the correctness of the last statement in this summing up of the author's conclusions. But we think that it is sustained by the most abundant proof. Besides that which is adduced by Sir John Forbes, we would allude to a grand fact which stands out very prominently, as we take a survey of the contentions, both in and out of the profession, in relation to the comparative efficacy of different remedies and modes of practice. It is the fact that a show of success is made out by the advocates of each of these rival remedies and modes. What is the explanation of this? Simply that the difference in the results is so small that it is difficult to ascertain it. It cannot be ascertained without both an extensive and a minute examination of the facts; and such an examination can seldom be had. The reason of the smallness of the difference is to be found in the ever present and effective agency of the recuperative power, which almost always towers above all the instrumentalities of art, and has so much to do with the cure of disease that, whether these instrumentalities have much power or little or none, there is no palpable difference in the gross results. When our therapeutics shall become what it should be, by the investigation which is now going on in regard to the relations of Nature and Art in the treatment of disease, the difference between good and bad practice will be more easily seen than it is now, simply because it will be so much greater. And yet, however great shall be the improvement in the prevalent practice of educated physicians,

the difference between its results and those of bad practice, whether ridiculously inert, as Homœopathy, or shockingly active, as Thompsonianism, will never be so great as to forbid a false show of success on the part of the advocates of quackery.

It would be interesting to pursue this subject further, for it presents numerous points which it would be profitable to discuss, but we pass on to other topics.

The great object of our author is to prove the extent of Nature's power in the cure of disease. This is well, but it is not enough. Unless the investigation be carried beyond this point, there will be great liability to error. A license may be given to an indiscriminating reliance upon nature. Here we think is a decided failure in the work of Forbes. To guard against the error indicated should have been quite a material and prominent point with him. It is true that he has much to say about watching Nature and rendering assistance to her efforts whenever it is necessary; but he does not treat this point with the force and emphatic distinctness which it merits. The indolent in the profession who ordinarily satisfy themselves with a routine practice, will find the same ease, on becoming convinced of the truth of Sir John Forbes's views, in resigning their patients for the most part into the hands of Nature. There is nothing in his book calculated to impress upon them the imperative duty of a strict watch, for the purpose of securing every opportunity that may offer for an advantageous interference of Art. This is a material defect. For, although Nature is the chief agent in the cure of disease, and the duty of the physician is therefore for the most part to watch for the purpose of gently assisting her operations, yet it is a great neglect of duty if he let slip any of the opportunities that occasionally present themselves of effecting decided changes for good by active medicine.

This defect in our author's presentation of the subject is seen in the view which he gives of different modes of practice. He reckons four prevalent modes.

1. *Extinguishing Treatment.* The object in this mode of treatment "is to *cut short*, as it is called, or extinguish diseases by a bold stroke, or *coup de main*, applied to them, or rather to the system, in their earlier stages." The grand means of doing this are bloodletting, emetics, purgatives, the cold bath, &c. Of this treatment he well remarks that

> "In rare instances the violent artificial disturbances excited in the system seem to overcome the natural disturbances existing in it; but, as a general rule, the indication is not only not fulfilled, but the existing disease is either aggravated directly, or the natural restorative powers of the system are arrested, enfeebled, or misdirected."

Though this mode of treatment was once in quite common use, our author says very justly that it "is to be abandoned as one of the forms of general practice; or, if employed at all, only to be so in very special and exceptional cases."

2. *Active Treatment.* In this mode the same means are used as in the first mode, but in a less active and more continuous manner, the object being to diminish materially the intensity of disease and check or retard its progress. This practice, formerly so prevalent, has become much modified of late, many of its advocates having "abandoned several of its most heroic weapons." In relation to this our author says that "the value of the practice increases in proportion as it recedes from the high heroic, and approaches the level of the more patient and humbler modes of practice to be noticed under the next head."

3. *Auxiliary or Mild Treatment: rational expectancy.* This system of treatment "recognizes the autocracy of nature in the cure of acute diseases, and proceeds on the principle that it is not only useless, but injurious to attempt to suppress or greatly to modify the morbid processes by strong measures of a perturbative or exhaustive kind." Sir John says further:—

"In attempting to estimate its value as one of the representative forms of the medical art, I would say that, while it possesses a very positive superiority over all the plans of treatment which, whether intentionally or not, virtually at least, leave Nature to her own resources, it presents an equal superiority over all the plans that endeavour to supersede Nature's proceedings altogether, by substituting for them the operations of Art. It occupies the lofty medium between doing too little and doing too much."

4. *Negative or totally Inert Treatment: pure expectancy.* While Homœopathy—that is, so long as it adheres to its infinitesimal doses—is an example of this mode of treatment, "in all the times of Physic it has been adopted, in a more or less rigid form, by many wise and experienced men;" and, adds our author, "I, for one, while disapproving it on principle, am disposed to think that it has fared worse with the sick since it was entirely set aside for the heroic system of treatment which has been so long prevalent."

In reading the remarks which Sir John Forbes makes in regard to these modes, the impression which we get of the comparative practical estimate which he puts upon them is in some respects indefinite. While there are some expressions which would lead us to think that he would admit the three first, or at least the second and third, into his practice, giving, however, a most decided prominence to the third, he certainly places them in strong contrast with each other, and speaks of the third as the one which he has long *preferred*, conveying the idea that he had discarded the others. He should have been more explicit. If he relies chiefly on the third mode, with an occasional admixture of the second, and little or none at all of the first, he should have said so. And if he had said so, we think that he would have stated the truth in regard to the comparative value of these modes. He should have gone further than this. While he condemns in glowing but just terms an exclusive heroic practice as so disastrous, that patients had better be wholly consigned to Nature's care than be subjected to its perturbing and exhausting processes, he should caution most positively against an entire abandonment of this mode, and should enjoin upon the practitioners that listen with such deference to the wise lessons of his abundant experience the imperative duty of resorting to active treatment whenever it is clear that it will aid in the recovery of the patient.

Another defect which we find in our author is the want of a proper appreciation of our means of acquiring a knowledge of therapeutics. In various parts of his book he speaks of the necessity of a Natural History of Diseases; that is, a description of diseases as they would appear in their various stages without any influence from remedies. He thinks that ignorance on this subject, which he believes to be exceedingly prevalent in the profession, is "the grand fundamental obstacle to the attainment of a true appreciation of the powers of Art," and cites the following as the chief causes of this ignorance:—

"1. The first and most important of these causes, is the defect existing in all medical schools, of having no distinct chair for instruction in the Natural History of Disease, and the consequent neglect of it, or very inadequate attention given to it by the professors of Pathology, Physiology, and of the Theory and Practice of Medicine, in whose courses alone any consideration of it could be expected.

"2. Another and equally important cause is the impossibility of young men being able to prosecute by themselves such a study, while still students, owing to there being no open field for it in hospitals or dispensaries; every case there presenting itself being seen under the influence of medical treatment.

"3. Again, great difficulties exist in the way of the hospital or private practitioner, of creating for himself a proper field for such a study. Of these difficulties we need only here notice the following: *a.* The practitioner's own conscientious scruples against leaving any cases to the unassisted care of nature, from natural doubts or fears—magnified by his previous teaching—that he might thereby be injuring his patients or even endangering their lives. *b.* The non-conviction or non-perception of the utility of the knowledge to be so acquired, preventing the suggestion of such trials; a natural result of his previous education, which inculcated the exclusive action of art in the cure of diseases. *c.* The dread of being exposed to the charge of mal-practice or dangerous practice, in the eyes of his pupils, or brethren, or patients, if he omitted the ordinary remedies in severe diseases, and trusted to the natural restorative powers of the system.

"4. It will also be admitted that since the great increase of charitable medical institutions in all parts of the country, it is not easy to meet with cases, even among the lowest classes, entirely left to nature; while, from the natural instinct to seek relief from suffering, the rich of all ranks almost invariably place themselves under some sort of treatment, regular or irregular; and there is never found any deficiency of practitioners of both classes who are ready to give their services.

"5. Finally, we must not pass over, as an uninfluential cause, the inability of practitioners, from prejudices of education, to see in cases treated by them with remedies either inert or adverse to a cure, the results of the spontaneous operations of nature, whether favourable or unfavourable; and the consequent inability to profit by the examples before them."

Much of all this is true, but some of it we dissent from most decidedly. We think that to some extent a false view is presented of the objects and modes of medical observation. We do not believe that it is right for a physician to forego, in any case, the use of positive medication where it is clearly applicable, merely for the sake of seeing how the disease will proceed under the guidance of nature alone. It is his duty to his patient to use all the means which can do him good. And we do not believe that any neglect of duty is required in any case for the purpose of accomplishing the object contemplated by Sir John Forbes. A knowledge of the natural history of diseases can be acquired without any such objectionable means. In mild cases, requiring almost no treatment, and in severe cases treated with inert remedies, falling under our observation accidentally, we can observe the uninfluenced morbid processes in such a way as to make out the natural history of diseases. And some, nay much assistance can be derived from cases which are subjected to treatment. In order to know what nature can do, it is by no means absolutely necessary to leave cases entirely to nature. The interference of art, even when quite decided, is compatible with the acquisition of this knowledge. And after all, what the physician most wants to know is, not what nature can do alone, but *how nature and art can work together* in the cure of disease It is to this point especially that his observation is to be directed.

The proposition which Sir John broaches of having a chair devoted especially to instruction in the natural history of diseases, distinct from the chair of practical medicine, is therefore wholly unwarrantable. There should be no such separation between the observation of disease left to nature and that of disease treated by remedies. Therapeutics, though wide in its range, is indivisible. It comprises both nature and art, and its success depends upon

a knowledge of their relations to each other in the cure of disease. Indeed, it can claim the resources of nature as its own, for its duty is to use and to regulate the natural agencies, to remove any obstacles that hinder their operations, and to supply their deficiencies. It can be called no invasion of the domain of nature for therapeutics to enter it. There, indeed, must be the great field of its labour, where it must for the most part co-operate with nature, but sometimes modify her movements, and now and then oppose and subvert them. It is only this broad view of the office of therapeutics that will insure the thorough and effectual investigation which will redeem our art from its uncertainties, so far as it can be done, and place it upon its proper basis. No partial views, however much of good they may accomplish, can lead to this grand result.

Another defect in this book is its failure to indicate the rules which should guide the investigations of physicians in discovering the complicated relations of nature and art in the cure of disease. The author has confined himself too much to the bare demonstration of the power of nature. He has laboured well in exposing error, but has not done as much as he ought in establishing truth in its place. He has shown most clearly that much of the practice still in vogue in the profession is not only useless, but positively injurious; but he has failed to point out, with sufficient compass and definiteness, the practice that should be substituted for it. He has introduced into the mind of his reader large but rather vague ideas of the curative powers of nature; but in relation to the power of art, he has done little else than inculcate cautions against any busy interference on her part. What the practitioner wants to know is, not merely that nature has great power, but in what ways he can co-operate with her, and under what circumstances it is right for him to interfere with her operations. Hints, it is true, are thrown out on these points (quite freely on the first point, but very faintly on the second); but the author should not have been satisfied with this—he should have made the points indicated prominent in his book, and showed definitely what rules should guide the physician in determining the limitations of art in each individual case, as it presents itself for his treatment. Failing to do this, Sir John Forbes leaves the subject somewhat at loose ends, and so the good service which he will render to the profession will be more limited than it otherwise would have been.

The author deserves the thanks of the profession for his clear and forcible exposure of some of the errors which have hindered and still hinder the progress of our art. We will give here an example of his mode of dealing with such subjects. There is one kind of practice very prevalent which he denominates the *pseudo-specific* treatment. The error of those who follow this plan consists in supposing what is true of a few remedies in relation to some diseases to be true of all the prominent remedies, and in relation to a great number of diseases. Of this error the author thus remarks:—

"In no department of science or art, in no phases of human action involving ratiocination and inference, are more false grounds admitted or more false conclusions drawn, than in this case; and the practical results are such as might be expected to flow from such a source. Every drug that has at any time been regarded by anybody as possessing some special power, either in curing diseases or in influencing the functions of the organs in which they are supposed to have their seat; every drug that has been recommended by anybody, more especially by authors and teachers, simply as beneficial in certain diseases, though on no better evidence than that it was employed in cases that recovered; every drug that has been proposed by writers as of probable or even possible utility, on mere theoretical grounds; every drug that has been sug-

gested by analogies, however vague; every drug that has not been previously prescribed in the particular disease in hand; in a word, almost every drug in our overflowing Materia Medica, whether inert or active, has been, on some ground or other, copiously prescribed in every variety of disease, under the supposed sanction of this pseudo-specific or empirical indication."

This kind of practice, Sir John says, "is at this very time in as great vogue as ever, although its employment may be often veiled under the technicalities of newer science. Nor is it confined to the ignorant or inexperienced among us, but adopted and followed by men of the greatest abilities and greatest eminence in the profession." This must be an over-statement. We know that it is not true of the profession in this country, and though the profession in England is not advanced as far as it is with us in casting off the over-drugging system of practice, so much in vogue some twenty-five years ago, we must believe that it is an over-statement in regard to British physicians. Here, also, is another statement, the strict truth of which we doubt. After speaking of advances which have been made in medicine, our author says: "It is very remarkable, however, that in regard to one most important part of the history of diseases, that, namely, of their natural course and event, infinitely less progress has been made; insomuch that it may now be affirmed that the practitioners of the present day are, speaking generally, almost as uninformed in this particular as were their predecessors fifty or a hundred years back." We hardly see how he could have been led to make such a statement. To say nothing of other diseases, do the profession at large know nothing more of the natural course of the exanthemata and other maladies to which Bigelow has applied the appropriate term "self-limited" than their predecessors did?

The truth is that Sir John Forbes, in his eagerness to make out his points, is rather prone to over-statements. He has shown this before in his famous article on "Homœopathy, Allopathy, and Young Physic," published in 1845. Among other things which he said, he uses this language about the general condition of practical medicine at that time: "*Things have arrived at such a pitch that they cannot be worse; they must mend or end.*" So far from this being true, things were at that time past their worst—perhaps we may say far past it—and were rapidly mending. Many, very many thinking minds in the profession had before that time discarded the prevalent heroic medication, and were exerting a great influence upon medical men generally. It is true that the article of Sir John (then Dr.) Forbes was not very well received by the profession generally; but it was not because he told them wholesome truths. It was rather because, in his earnestness, he often went beyond the strict truth, and especially because, in doing so, he made some unwarranted admissions in regard to Homœopathy and the founder of that master-piece of delusion and quackery. When he asserted that Hahnemann's name "will appear in the history of medicine in the same list with those of the greatest systematists and theorists," and actually associated it with the names of Stahl and Boerhaave; and when he further said that his system of medicine was "destined probably to be the remote, if not the immediate cause of more important fundamental changes in the practice of the healing art than have resulted from any promulgated since the days of Galen himself," it is no wonder that the profession generally should be offended by opinions so palpably groundless, and should therefore receive with suspicion the sentiments of him who uttered them. But notwithstanding he thus unnecessarily awakened opposition in the minds of the profession, all that is true in that article was appreciated by all rational physicians, and the article had really

a decided influence in hastening the downfall, then already begun, of the reign of heroic medication. And the work before us, although it has many defects, will do vastly more than his article did towards effecting this object.

Although Sir John Forbes puts the name of Hahnemann, that wildest of all medical dreamers, in the same category with that of Stahl, he in one part of his book eulogizes Stahl, and says of him that "his works contain more of original truth in regard to the nature and treatment of diseases, and in regard to the proper method of studying them, than any other writings of his time." He says, also, that the system of treatment which he advocates is exactly that which Stahl recommended a century and a half ago—viz., that of *mild auxiliary treatment*, or *rational expectancy*. It would seem that Sir John really laments that the inert practice of the times of Stahl, with its mass of complex and inefficacious compounds, ever gave way to the active medication introduced by Cullen and his immediate successors. It may be that the change was not immediately beneficial; but we have no doubt that it has been remotely so. In order to place therapeutics upon a rational basis, it was necessary that it should pass from the inert polypharmacy of the times of Stahl through a stage of undue activity. A too large use of powerful remedies was a necessary evil, incidental for a time to the introduction of these remedies to the knowledge of the profession. The result is, that these remedies, used too freely at first, from the very fact that they are so valuable, are now coming to be used with the proper discriminations; and their value is such, that we have no hesitation in saying that the substitution of the judicious use of them for the aimless and frivolous polypharmacy of the times of Stahl, is worth vastly more than has been paid for it in the evils which have been induced from the heroic medication through which the benefit has come. At any rate, we do not believe that even Sir John Forbes would seriously advocate our going back to the Stahlian practice, with all its encumbrance of useless material, and its opposition to the use of such remedies as cinchona and opium.

We would remark, in this connection, that we hope there will soon be an end to purchasing experience at so dear a rate as has been usual in our profession. It is time that more strict and rational rules of investigation should govern in practical medicine. There should be vastly more caution in the use of all remedies than is common with the great majority of medical men. Especially should this be so with all new remedies. The habits of the profession generally need to be entirely changed on this point. It is the dictate of common sense that a new remedy should be used sparingly at first, and its operation should be submitted to the closest scrutiny, before it be brought into extensive use in the profession. But the reverse of this has been the case with most physicians. It is the few only that have been cautious at the outset. Accordingly, every new remedy has at once, immediately after its announcement, been brought largely into use; and after experience has been had, either of its inefficacy or of the dangers attendant upon its power, its use has become limited, or has been given up. So, too, for the same reason, the young physician generally uses medicines with a free hand; but as he grows older, his experience of their results leads him to greater and greater discriminations and limitations in their employment. This, too, should be reversed. The young practitioner, with his limited practical knowledge of medicines, has need of more caution than is necessary after he becomes thoroughly acquainted with the effects of the means which he employs. The range of his medication should rather widen than narrow itself as his experience increases, if he start right in the beginning.

Although the heroic medication, which was so prevalent in the first quarter

of this century, has of late years been to a great extent relinquished by a large portion of the profession, there is still so much of undiscriminating and incautious drugging, that there is great need of such efforts as Sir John Forbes and others have put forth to arrest it. We hope that the subject of his book will continue to be agitated. *It is the great subject for the consideration of the profession at the present time.* Whether our therapeutics shall come out aright from its present transition state, depends upon the manner in which this subject shall be investigated. If it be examined in the proper manner by those who write upon it, and by practitioners as they go about in the daily rounds of their practice, there will be greater advances in practical medicine in the next twenty-five years than have ever been made in the same length of time before. We have said that the presentation of the subject by our author, though very full, is yet in some important points defective. Let others, each in his own way, pursue the discussion, so that the subject may be presented in every variety of phase, and it will then be fully developed, all defects being supplied and all errors removed.

The great diminution of positive medication during the past twenty or thirty years has been attributed, by some, in a great degree, to the influence of homœopathy. As the reader has already seen, Sir John Forbes goes beyond all other physicians in this opinion. That the demonstration of the capability of nature to cure disease, made by the followers of this infinitesimal practice, has had some influence in bringing about the result referred to, we most cheerfully allow. But its influence has been incidental and secondary. Homœopathy has merely aided a movement which originated from causes existing in the profession—causes which had begun to operate long before this system of quackery had obtained a foothold in the community. And it was no part of Hahnemann's design to do the good which he has done to the profession. He did it unwittingly. He had no belief in the curative power of nature. It was medicine "dynamized" by trituration and a certain number of "downward shakes," by which, in his opinion, disease was to be cured. He had not the remotest idea that the diffusion of his system was to aid the profession which he affected to despise in carrying on a reform that had already fairly begun. He did not discover that as his system should pass away, as all other exclusive systems had done before it, it would leave, as a valuable residuum, abundant evidence, in connection with that which medical men had been themselves collecting, of the grand truth that nature is the chief curer of disease. Though we can think of him only as an arrant impostor, we are thankful, on account of the good service which he has done the cause of practical medicine, that he was permitted to live, notwithstanding the evil which the temporary popularity of his system has occasioned.

There are many more points in the book of Sir John Forbes which it would be interesting to notice. But it has not been our object to write a full review of it, but to touch upon some points which particularly required our attention, on account of their importance, or on account of some errors which we had found in his treatment of them, leaving the reader to resort to the book itself for a knowledge of all the author's views. And we would repeat the wish that the book may be reprinted in this country and have an extensive circulation.

W. H.

ART. XV.—*Cours Théorique et Clinique de Pathologie Interne, et de Thérapie Médicale.* Par G. GINTRAC, Professeur de Clinique Interne, et Directeur de l'Ecole de Médecine de Bordeaux, etc. etc. etc. Tomes 1, 2, 3. Pp. 669, 696, 840. Paris, 1853.

A Theoretical and Clinical Course of Internal Pathology and Medical Therapeutics. By G. GINTRAC, Professor of Internal Clinics, and Director of the School of Medicine of Bordeaux, etc. etc. etc. Volumes 1, 2, 3.

THE three volumes before us are the commencement of a system of scientific and practical medicine, which, if completed with the same ability as is evinced in the preparation of these, cannot fail to rank among the most erudite, comprehensive and valuable that have heretofore appeared. It will form, in fact, a comprehensive system of general as well as special pathology and therapeutics.

The work opens with a series of instructive preliminary remarks on the nature of medicine; its origin, first as an empirical art, and subsequently as a science based upon determinate principles; with a general consideration of the bases upon which it rests, the obstacles that impede, and those which have favoured its progress. Embracing under the latter head a rapid sketch of those who, by their labours, whether as observers or investigators of disease, its nature, causes, phenomena, and treatment, as collectors and systematic compilers of facts contributed by others, or as writers and teachers, have exercised a powerful and permanent influence in promoting the onward march of medical science and practice; of the various institutions—schools, hospitals, clinics, scientific associations, periodical publications, and museums of pathological specimens, that have been productive of a like result; and, finally, of the aids afforded in the improvement of the healing art by the progress of the accessary sciences, anatomy, physiology, natural history, comparative pathology, chemistry, physics, astronomy, and general philosophy.

This division of the introductory portion of the work terminates with a consideration of the degree of certainty, utility, and dignity to which medicine, as a science and an art, may rightfully lay claim.

The succeeding division is devoted to a sketch of *bionomia*, or a brief summary of the phenomena and laws of the organism of man in health, as an introduction to the study of human pathology.

The generalities of pathology are then considered, comprising a general idea of disease, its definition, causes, symptoms, progress, varieties, complications, terminations, seat, nature, semeiology, diagnosis and prognosis, including a general notice of pathological anatomy, chemistry, microscopy, and physiology.

The first volume concludes with a consideration of the generalities of therapeutics, under the heads of, 1. The different therapeutic methods—the expectant and active, the natural and perturbative, the rational and empirical, the synthetic and analytic, the etiological and symptomatic. 2. The therapeutic indications and agents. 3. Plans of medication: 1—the direct, excitant, general, special; 2—the direct, sedative, general, and special; 3—indirect medication, excitant, and sedative; 4—specific medication.

The second volume commences with the study of diseases in general. These M. Gintrac arranges in three grand divisions. In the first, he includes congenital lesions—monstrosities. In the second, the lesions produced by

mechanical, chemical, or poisonous agents; and in the third, vital and organic lesions.

The consideration of the lesions included in each division is followed by a general sketch of their respective therapeutic management.

The lesions of the third division are treated of under two heads—1. Elementary affections, or morbid conditions: *a.* Principally dependent on the abnormal mode of action of the solids: *b.* Consisting in alterations of the fluids, and especially of the blood: *c.* Resulting from a general alteration of the solids and fluids—*diatheses*—with their general treatment. 2. Determinate diseases; divided into seven classes—1. Congestions; 2. Inflammations; 3. Hemorrhages; 4. Fluxes, and alterations of the secretions; 5. Organic lesions; 6. Neuroses; 7. Fevers.

"This arrangement," M. Gintrac remarks, "has been purposely adopted. I have desired to place in the first rank the local diseases, which may be regarded as simple and primitive, and terminate with those which are more general and very often consecutive to the others. I have had a further motive in placing fevers in the last rank. In the consideration of this class of diseases I have been obliged to enter into descriptions and observations which in some measure appertain to the domain of special pathology. The transition from general anatomy to the latter, will thus be rendered more natural."

The 5th class, that of organic lesions, is divided into *four* orders: 1. Lesions which consist in a notable change in some one of the physical properties of an organ, without modification of structure. 2. Lesions which consist in a change of structure from the occurrence of abnormal interstitial formations. 3. Lesions produced by the formation of independent organisms—*entozoa, helminthia.* 4. Lesions resulting from a local extinction of life—*gangrene.*

The class of fevers is not completed in the third volume.

The last portion of the present treatise, which M. Gintrac informs us will be the most considerable, and the most essentially clinical, will embrace the special lesions—in other words, those of the external sensitive apparatus, of the encephalic and nervous, of the locomotive, of the vocal and respiratory, of the circulatory, of the digestive, of the secretory, and of the genital apparatus.

The plan laid down by M. Gintrac is a most extended and comprehensive one, and arranged in a manner that strikes us as most skilful and rational, facilitating the acquisition of the principles and practice of medicine by gradually descending from the general to the special, from the simple to the complex. We trust that the author may be spared to complete the entire plan of his treatise in the same spirit in which it has so far been carried out. That he is fully competent for the satisfactory fulfilment of the important task he has assumed, the volumes before us bear ample testimony. Deeply versed in the literature of his profession, and, consequently, familiar with the vast fund of medical knowledge accumulated and perfected by the successive labours of the industrious and talented physicians of almost every clime and age, and prepared, by ample clinical experience, and habits of close and cautious observation, acquired under the most favourable circumstances, to apply this knowledge to the successful preparation of a course of scientific and practical pathology and therapeutics, we have no fear that the succeeding volumes of the present treatise will not equal those we have had an opportunity to examine.

In all his teachings M. Gintrac is clear, definite, and concise; entering, at the same time, sufficiently into details to present a fair and full exposition of the present state of medical knowledge in relation to every subject of which

he treats, and to impart precise and accurate ideas of every point of importance in his exposition of disease and its management. He is evidently more desirous of presenting well established facts and principles than of dealing in subtile discussions, or of acquiring for himself the doubtful praise of originality in his theoretical or practical views. Upon mere hypothetical questions, or those of doubtful profit, he never dwells, but coming direct to what he believes to be legitimate doctrines, established upon well observed and carefully digested facts, whether in the domain of pathology or therapeutics, his teachings have a character of simplicity, combined with force and truthfulness, that render them particularly adapted for the profitable instruction of the advanced student of medicine, as well as attractive to the practitioner who is desirous, on any particular point, to test or compare the result of his own observations with that of preceding and contemporary authorities in the profession.

The prelections of M. Gintrac are, moreover, all of a positive character. Even on questions as yet unsettled, or in relation to which different or even opposite opinions are entertained by eminent members of the profession, each claiming to be sustained by positive facts and direct observation, the difficulties in the one case and the discordant views in the other, are so clearly and concisely presented by him as to enable the reader to form a correct conception of the first, and assist him in judging of the amount of probability in favour of the correctness of one or other of the latter.

We do not pretend to say that the work of M. Gintrac is without fault. We should not, certainly, feel inclined to indorse all the views advanced in it in relation to pathology, nor every direction given by the author for the treatment of disease. We should be, nevertheless, willing to give to it our unqualified recommendation as a full and well arranged system of the principles and practice of medicine—presenting a very fair exposition of generally received doctrines, recognized facts, and established plans of treatment.

We may remark that M. Gintrac is among the few continental writers who are willing to give credit to the physicians of this country for the contributions they have made to medical science and practice.

That our readers may be enabled to form some idea of the manner in which M. Gintrac handles the several subjects included within the scope of the present course of pathology and therapeutics, we translate the following sections from the part devoted to the consideration of fevers. We have selected these, not because of the particular excellence of the views set forth in them, but because they refer to an important question, upon which medical writers of the greatest eminence have widely differed; namely, that general idea of fever, which shall at once convey a clear conception of the disease in its true pathological acceptation, distinguishing it from all other affections with which it may be associated or complicated. To the selection of these sections we were prompted also by the difficulty of finding any others sufficiently short for our purpose, and in which the subject treated of is presented with the same degree of completeness.

The first selection is from the section devoted to a consideration of the general idea of fever.

"Increased heat was, with the ancients, the principal sign—the essential character of fever. There are, however, fevers in the course of which the temperature of the body, far from being increased beyond the normal standard, does not reach it, or even remains far below it. Such is the case in the fevers denominated *algid*. In the first stage of intermittent fever also, there is a sensation of cold. Often the temperature is so little changed that scarcely can the hand of the physician discover any difference. In order, therefore, to form an

exact idea of fever, it is necessary to have recourse to some other characteristic.

"According to Stahl, fever, considered abstractedly, consists in a modification of the course of the blood, especially in an acceleration, and a certain abnormal direction of the circulation. Boerhaave, in the same manner, indicates the frequency of the pulse as the most constant phenomenon of fever; the one which denotes its invasion, and which ceases only with it. Quesnay and Dehaen adopt a similar definition. Piquer has remarked, and very justly, that, for the physician to detect the presence of fever, it is not necessary for him to be acquainted with its essence or its mechanism. Embracing with his fingers the wrist of his patient, and compressing lightly the radial artery, he thus perceives, at the same time, the heat of the skin and the frequency of the pulse. He is at once enlightened, and enabled to decide upon the presence or absence of fever.

"The physician would nevertheless be led into error, were he in every instance in which the pulse is accelerated and the skin hot to conclude that fever is present. An individual may have experienced a violent mental emotion, or some other powerful excitement; he may have partaken freely of stimulating food, or of alcoholic drinks, which, particularly if he be young and of an excitable temperament, would be sufficient to cause an acceleration of pulse, and lead to the supposition that he was actually labouring under fever, when, in fact, he had not passed from a physiological state. There are also organic lesions of the heart that produce frequency and irregularity of pulse, without the actual presence of fever; in such cases, however, the presence of other symptoms would reveal the existence of the serious lesion by which the disturbance of the circulation is produced.

"With these and some other somewhat similar exceptions, to be noticed hereafter, frequency of pulse may be received as the most certain sign of fever.

"We are not, however, to suppose that an idea so precise and simple as this has been generally adopted. Stoll says that in fever there is an *alteration* of the pulse, and a lesion of one or several functions. The expression alteration is an extremely vague one. Selle is still more indefinite. According to him, fever is a disease variable in its course and duration, and accompanied by heat, cold, and a pulse sometimes more frequent, at others slower than during the ordinary state of health. Afterwards, however, he is more precise, when he pronounces fever to consist in variations of temperature, frequency of pulse, spasm of the heart, and acceleration of the circulation of the blood.

"Grimaud objects to the definition of Boerhaave, because the determinate character of fever laid down by the latter is often absent. Thus Galen, Sarcona, and Worlhoff describe fevers in which the pulse was slow. Grimaud, however, distinguishes frequency of pulse—that is, the number of pulsations in a given time—from celerity or quickness, which relates to each separate pulsation, which may be completed in a variable period of time. Thus the pulse in fever may be quick, but not frequent. Fordyce and Boisseau consider it inaccurate to assume acceleration of pulse as the pathognomonic sign of fever, inasmuch as it is observed in diseases that are non-febrile, and is sometimes absent in genuine fevers.

"All this discrepancy of opinion results from the fact that certain morbid conditions, certain groups of symptoms, have been classed with fevers, although independent of them. Thus, hydrophobia is ranked with the fevers by Selle, while it actually belongs to the neuroses. The acute ataxic condition has been assimilated with the nervous fevers, although frequently unattended throughout with fever. Certain apyretic periodical affections have been ranked with intermittent fevers, under the denomination of *fèbres larvatæ*; it being supposed that in these fever was actually present, but, at the same time, veiled or masked, so as not to be detected exteriorly. Let us, however, aim at something more definite. When the pulse is normal in respect to frequency, is there fever? Do there exist fevers without fever? Is fever one of those mysterious beings which are sometimes apparent, and at other times, though present, invisible? To have recourse to such a supposition, is to create the purest ontology. How loudly would the partisans of the physiological and organic doctrines exclaim, and very justly too, against it.

"Let us avoid, as far as possible, doubtful conceptions. The practitioner has need of a precise formula, of a definition exact and well understood, of a definition clear and recognizable. For him there is fever whenever the heat of the skin is modified, and especially when it is augmented; and when the pulse is evidently more frequent than in the physiological condition. To these characteristics others are added, as thirst, spontaneous lassitude, etc.

"What do this acceleration of the pulse, and this increase of heat, which constitute the characteristics of the febrile condition, denote? With what form of organic or functional lesion are they connected? They result, evidently, from an augmentation of action in the heart and bloodvessels; from the disturbance and stimulation of the organs of the circulation. This has been correctly remarked by many authors. According to Quesnay, fever consists, radically, in the excess of action in the arteries. Stoll expresses himself thus, in his seventh aphorism: 'The phenomena of fever are deducible from the irritability of the heart and arteries, augmented and excited by some stimulation.' Ludwig speaks in these terms: '*Circulatio sanguinis et humorum aucta febris dicitur. Febrium indoles et vis in circulo sanguinis cernitur.*' Wedekind, of Mayence, and his pupil, Herzig, define fever 'disease of the entire system, resulting from an abnormal activity of the heart and bloodvessels, excited and maintained by an internal cause.' Benjamin Rush places its seat in the bloodvessels, and defines it to be an anomalous action or a convulsion of these vessels. Rolando refers its production to cardiac excitement.

"Thus, according to numerous observers, fever is an affection of the circulatory apparatus. It is the augmentation of action, the general excitation or hypersthenia of the vascular element. To this idea, deduced from the closest scrutiny of the phenomena of fever, there have been joined considerations of a more recondite character. Thus, fever, or the excitation of the circulatory apparatus, is viewed as a *reaction* provoked by some agent which disturbs the vital harmony. It is, says Fages, the result of the reactive efforts of nature against the impression of a morbific cause. According to the physicians of the school of *anamists* and *naturists* it is for the purpose of purification that fever establishes itself; hence, they give, as its etymology, the Latin word *februare*, to cleanse, to purify. The perspirations, the different evacuations by which the accessions of fever are terminated have given an appearance of truth to this opinion.

"The febrile reaction, with the conservative and critical efforts which it induces, demonstrate, according to Stahl, the intelligence of nature, the autocracy of the soul, while, in the opinion of M. Cayol, it constitutes even a function.

"After all, however, fever is in truth nothing more than a morbid condition. However advantageous may be, in some instances, its results, it commences invariably by a serious derangement of the organism, a general disturbance of its functions. This perturbation has not always a beneficial end, a favourable result. Too often it destroys the strength and exhausts completely, in fruitless agitations, the resources of the organism."

The second section we shall translate is that which treats of the distinction of fever into essential and symptomatic.

"We conclude, then, from what precedes, that fever consists in a more or less intense excitation of the circulatory apparatus. In all cases in which we cannot discover any serious lesion in any organ beyond that apparatus, the effective disease—that is, the fever—must be evidently essential or idiopathic. If, on the contrary, we recognize in any part of the organism any lesion, under the influence of which the fever has originated, we should then consider it as symptomatic or secondary.

"This distinction, presented thus generally, appears clear and evident, but, in its application, there occur some difficulties.

"Galen saw in fever, sometimes the result of an alteration of the humours, sometimes the result of an inflammation. Baillou separated, distinctly, essential from symptomatic fevers. Guittierez discusses this question: '*Sit no acci-*

piendœ febris divisio in morbum et symptoma? and resolves it affirmatively, in employing all the syllogistic formula so much in vogue during his time. Until Pinel, little attention was paid to this distinction of fevers, which was regarded as scholastic. Selle embraced in the class *pyrexia* the most part of the acute phlegmasia, the exanthemata, many organic lesions, and even some of the neuroses. Our celebrated Nosographer, to avoid this confusion, banishes from the domain of pyretologia all those diseases alien to it, and lays down the principle that essential or primitive fever may present itself under certain determinate forms, and is subordinate to the organ principally affected. Rejecting the idea of a simple fever, he refers inflammatory or agiotenic fever to the blood-vessels; bilious or meningo-gastric fever, to an affection of the stomach; the mucous or adéno-méningic, to the alimentary mucous membrane; the adynamic fever, to the muscular system; the ataxic fever to the nervous system, and the adéno-nervous, to the glands and the organs of innervation.

"The greater part of these denominations, and the various local seats of fever indicated by them, led Pinel, without his intending to do so, to rank fever not as an essential disease, but as a morbid condition dependent upon a lesion of different parts of the organism. In following the principles adopted by the author of the *Nosographie philosophique*, we are led to a complete overthrow of his doctrine. It was, in fact, early opposed. Cortambert opposed the denomination of essential fever. Caffin rejected such fevers, and referred them all to a local affection. Bravet followed, by an analysis of each special order of fevers, and showing their particular localization. Parkinson goes still further; he would erase from nosology the very term fever, as never expressing anything beyond a symptom. Subsequently to the year 1804, Prost had collected a great number of clinical and necroscopic observations, which prove the direct relation of the different orders of fever, and especially of the ataxic fevers, with divers phlegmasia. Hence, when Broussais came to aim the final blow that was to overthrow the pyretological system of Pinel, the medical mind was prepared for such revolution; it nevertheless required an athlete like the professor of Val-de-Grace to triumph over a doctrine which had been looked upon as invulnerable. But, it is not alone the partisans of Broussais who have opposed the essential character of fever. Fages, a learned disciple of the school of Montpellier, rejected the idea of a simple, essential fever. He denied that character to the fevers denominated inflammatory, bilious, and mucous, because that the inflammatory bilious, and mucous conditions may exist without fever. According to him, fever is merely a symptom, a reaction, and cannot, therefore, form the distinctive character of a class of diseases. In our day, similar opinions are professed by the celebrated Schoenlien, of Berlin. He sees in fever, nothing but a reaction—the participation of the economy in the disorder of an organ. It is not the disease, but only its shadow.

"This opinion is certainly an exaggeration. Fever constitutes often that which is the most essential or the most important in a disease. This must be admitted when, upon the most rigorous analysis, no actual disturbance can be discovered excepting in the circulatory apparatus; when this trouble is due to general influences, which have modified the entire organism without producing any local lesion; finally, when, on opening the body after death, we discover no trace of an alteration capable of explaining the fatal termination of the case, of which Chomel has furnished examples.

"Foquier and Lerminier, reporting to the Society of the Faculty of Paris, on a memoir presented, on this important subject, by M. Chomel, endeavour to determine in what the essentiality of fever consists. I quote their words, which appear to me to express views that are perfectly just.

"'The alteration of the animal heat and of the circulation is the result of an irritation experienced by the vascular system. Whether this irritation is confined solely to that system, or takes its source in all parts of the body, it will not be less essential, so long as it is not produced or kept up by an anterior or concomitant affection alien to the vascular system.' These skilful practitioners add this reflection: 'If loss of appetite, some pain of the epigastrium, and a slight redness of the tongue suffice to indicate the presence of gastro-enteritis, redness of the face, a sense of weight in the head, and cephalalgia, should, in

like manner, lead us to infer an inflammation of the brain or its meninges; and yet all these symptoms may quickly disappear, and, in every case, much more readily than would be the case were they dependent upon actual inflammation.'

"But fever often accompanies various local lesions—are in such cases the latter to be viewed as causes or effects?

"M. Camille Leroy has remarked, 1st, that the general phenomena most often are the first observed, while the local affection does not manifest itself until subsequently. 2d. That the causes act upon the entire nervous system, the whole circulatory apparatus; that they make their first impression upon the organic centres, upon the very sources of life, and do not give rise, but subsequently, to congestions and fluxions, which are the consequences, and not the motors of the febrile movement. I fear that by this mode of reasoning we shall be led too far. Do we not every day see pneumonia, or some other serious and incontestable inflammation preceded by prodroma and intense fever? The local phenomena do not present themselves with energy until at the end of some days. Should we, therefore, regard the local inflammation as the consequence of the febrile movement? It is from such a view of the subject that the acute exanthemata have been brought back among the essential fevers, and in this I believe that we have made a retrograde step.

"To avoid all confusion, we shall consider as essential fevers such only as present beyond the circulatory apparatus, phenomena that are of little import, variable, mobile, and of themselves incapable of exciting a reaction; but we shall refuse the title of essential to a fever from the period when there occur specific lesions, which exercise an incontestable influence upon the march of the disease. Should the fever, even when it originates in a determinate and perfectly recognizable local cause, be considered as a symptom of little value? No practitioner will reply in the affirmative. Take, for example, the most simple case, that of a wound, or of a fracture, by which fever may be excited, does this latter, when it occurs, merit no attention? Does it not become, by its intensity or its particular progress, an important basis of indication? When convulsions or tetanus occur as the result of a traumatic injury, are these formidable accidents, because produced by a local cause, to be overlooked, neglected as mere secondary phenomena? If in a case of tetanus or convulsions, the nervous system is disturbed, may not the circulatory apparatus be excited when fever is produced. Does not the one morbid condition demand attention equally with the other?

"From the various considerations that have been presented, I believe the following conclusions are to be deduced:—

"1. That the distinction of fevers into essential and symptomatic, at every period, heretofore admitted, should be retained.

"2. That the domain of the essential fevers has been too much extended, but that it would also be wrong to restrict it within too narrow limits, or to efface it from the nosological chart.

"3. That its true limits should be determined by observation.

"4. That the fever which presents itself only as a symptom, should, under certain circumstances, be taken into consideration as an indication of the active part taken by the circulatory apparatus in the local disease.

"Desirous of simplifying the subject in question, I shall avoid as much as possible in the succeeding consideration of fevers, all that relates to symptomatic fevers. The latter present, in effect, independently of the phenomena strictly febrile, those which appertain to the local affection of which it is the coincidence or effect. Hence, in respect to them, it is easy for a confusion to arise that shall impede the precise determination of the proper causes and attributes of fever properly speaking. The essential fevers should be studied apart from all others, for they may exist alone, as can easily be proved. They may, also, co-exist with other diseases of which they are independent, but upon which they exert an influence. A fever may, also, survive the local lesion that has given it birth, and take then the character of an essential disease."

D. F. C.

ART. XVI.—*Diseases of the Skin.* By ERASMUS WILSON, F. R. S. Fourth American, from the fourth and enlarged London edition. Philadelphia: Blanchard & Lea, 1857.

THE writings of Wilson, upon diseases of the skin, are by far the most scientific and practical that have ever been presented to the medical world on this subject. For many years a large portion of his time has been spent in the field of dermatology. His eminent position as a medical man in the metropolis of England has afforded him the most ample scope for prosecuting any investigations relating to the manifold abnormal conditions which the external investment of the human frame is known to exhibit. The results of these investigations he has proffered to the profession, by whom, without doubt, they will continue to be appreciated on both sides of the Atlantic. Mr. Wilson's style is in the highest sense elegant and classical; it is absolutely faultless. It possesses an individuality; it glows with a freshness and flows from a depth of thought which mark it as emanating from the rich fountain of his own brain.

The present edition is a great improvement on all its predecessors. In it the indefatigable author has arranged, in part, a new system of classification, founded, as he believes, on the only true basis, the CAUSE of the disease. To the consideration and elucidation of this subject one entire chapter is devoted. A new chapter is introduced relating to the *general pathology* of cutaneous maladies; and another, in which their *general therapeutics* are discussed in a very lucid and able manner. Moreover, we have a separate chapter on diseases of the *sebiparous glands;* and a brief but valuable article on the *malignant pustule* of the skin. The different *syphilitic eruptions* also occupy a prominent position in the work; and are arranged according to their forms and the period of their development in a table of classification.

Probably no branch of medical education at the present day is so sadly neglected, we might say ignored, as that which relates to a knowledge of the skin; and probably no department of medical and surgical practice is performed in so blind, so graceless, so culpable a style as that bestowed upon its maladies. Students look at the skin. Of course they do. They see that, like the clouds suspended over their heads, it presents various shades of colour between white and black. But, of its structural character, its anatomical elements, and of its deviations from health, they are well-nigh as ignorant as the untutored wild men of the forest. This, at least, is true of a vast number, who seek refuge and patronage under the prestige and shadow of a diploma, obtained at the fountains of medical science, that send their streams through the length and breadth of the land. Well, this destitution, or meagreness of knowledge, and its manifold direful consequences, accruing to thousands who apply for deliverance from some annoying cutaneous affection, may be effectually removed through the instrumentality of the brilliant, scientific light, reflected from the elegant pages of Erasmus Wilson. To one inclined to ponder upon the suggestive and instructive contents of these pages, with an attitude of mind open to drink in knowledge, it would seem as if the spirit of philanthropy, united with the genius of sound learning, and commiserating the sufferings of humanity, had prompted and qualified our author to present to his professional brethren a system—a code—a practical guide, at a most opportune epoch; and which, if they follow, will be to them what the skilful and faithful pilot is to a sea-tossed vessel, seeking anchorage in a strange harbor.

To dwell upon all the great merits and high claims of the work before us, *seriatim*, would indeed be an agreeable service; it would be a mental homage which we could freely offer, but we should thus occupy an undue amount of space in this *Journal*. We will, however, look at some of the more salient points with which it abounds, and which make it incomparably superior in excellence to all other treatises on the subject of dermatology.

The first chapter of the book is dedicated to a consideration of the minute anatomy and physiology of the skin. An exact description, couched in plain but graphic phraseology, is given of the several component tissues, which, in their united and associated capacity, constitute this most complex covering to the entire human organization. The mind of the reader is thus, in the very beginning of his study, placed upon a scientific basis, without which no real useful improvement can be made in the practical department to which he is subsequently introduced, and in which he expects to labour. To reach this latter goal, through the medium of successful study, is a noble employment for every medical student and for every practitioner of the healing art, and is sure to carry with it its own most welcome reward.

In pursuing his investigations of the normal structural anatomy of the skin, under the guiding hand of Wilson, the reader is agreeably surprised to find that the patient researches and observations of his teacher have shed a flood of light upon many obscure points that had obstructed his path in all previous investigations; and this light, this veritable knowledge which he has gained in regard to healthy cutaneous structure, is easily and directly applicable to the morbid anatomy or pathology of the skin; in other words, to the diseases that invade this tegumentary organ. For instance, Wilson has devoted much study to the mode of development and growth of the epidermis; and the curious results at which he has arrived, and which we can verify from our own investigations, are detailed in the work before us. They are extremely interesting, and materially aid us in understanding some of the anatomical features of lepra. The investigations we are now considering have educed the fact that in this complaint the primitive epidermic granules are abnormally formed, being in a state of hypertrophy from deranged nutrition of the derma.

The structure of the hair has received a due share of attention, and conclusions naturally flowing from these investigations are also presented for our consideration. The minute organization of the pilous tissue harmonizes with that of its analogue, the epiderma, being composed of the same primitive granules, differing only in their arrangement, as being destined to serve a different purpose. These horny appendages of the skin, that is the hairs, are produced by the involution and subsequent evolution of the epidermis; the involution constituting the sheath of the follicle, in which the hair is inclosed, and the evolution the body or shaft of the hair. Our author condescends even to the work of *splitting of a hair;* that is, his microscopical examinations lead to a natural division of a hair into a central portion, a peripheral extremity, the point; and a central extremity, the bulb or root. The coarsest hair is found in the female, and the finest in the male. This is the reverse from the general opinion. The habit of cutting the hair or shaving it, does not increase its size. The hair is composed of three different tissues—a loose, cellular tissue, which occupies its centre, and constitutes its medulla or pith; a fibrous tissue, which incloses the preceding and forms the chief bulk of the human hair; and of a thin layer of superimposed scales, which envelops the fibrous structure, and forms the external structure. The primitive granules of the hair are arranged in such regular order as to have the appearance of parallel fibres, and their variation in tint gives rise to diversity of colour in

the hair. Under the influence of disease the hair is liable to a change, which Mr. Wilson compares to the alteration of the epidermis which takes place in lepra—that is, it becomes lax in texture, brittle, twisted, and is robbed of its natural colour. The malady, of which we are told this morbid change is a symptom, is "*ringworm.*" An examination of the hair in this troublesome complaint exhibits its primitive granules morbidly enlarged, like those of the epidermic cells in lepra; both diseases being a granular degeneration of the cells of which the epidermal product is composed.

Another important and interesting discovery to which our author's investigations have conducted is, that the pigment of the choroid membrane of the eyeball and melanosis are composed of the same identical organisms.

Such are some of the examples of the first results of the application of scientific research to practical uses. Wilson, in the plenitude of his modesty, regards them as humble examples, but at the same time they offer inducements for other similar researches.

The plates, illustrating the work, are fifteen in number, eight being coloured after nature, the remainder being plain. Each of the coloured plates represents a group of diseases, and they will serve to assist the physician in forming a diagnosis of any given specimen he may encounter on the living subject.

The second chapter is devoted to a *Classification* of diseases of the skin. By those who are familiar with the previous editions of Wilson, it will be remembered that the nomenclature which he adopted went by the title of the "*Natural System,*" founded on the physiology and pathology of the skin; and took, as its leading features, the phenomena that wait on inflammation of the derma, as congestion, effusion, suppuration, desquamation, &c. In all essential particulars this arrangement coincided with the system of Willan. To regard diseases of the skin merely as essential affections, located in some one or more of the different component parts of the derma, while the others are supposed to be in health, is doubtless correct reasoning in many cases; but the theory is a hazardous one, and often a false one; and the attempt made by Wilson to remodel the system of Willan, and partially set up and build upon it the anatomical or "Natural System," was regarded by many as unsuccessful and fallacious. With this system our author himself was dissatisfied, and for many years his mind has been engaged in constructing a classification of a more practical and philosophical character. He has at last completed an *Etiological System* of arrangement, which was initiated by Hippocrates, and followed by Franck, Plumbe, Baumès, and others. The advantages set forth as appertaining to this system, and raising it above all others in the scale of excellence, are, that it renders the treatment of cutaneous maladies more simple and rational, and removes them from the special and isolated position which they now occupy, into the general category of diseases incident to the human frame. In some respects the present nomenclature of Wilson reminds one of the views entertained by M. Devergie, of Paris, who regards the determination of the precise dermic anatomical characters and seat of each particular disease as a matter of minor consequence, but looks at the pathological relations of cutaneous maladies with the conditions of internal organs, and of the system in general, as a matter of great importance, and to be always borne in mind. He divides all skin diseases into *secreting* and *non-secreting*—a striking and practical mark as far as it goes.

The cardinal principles laid down in the present edition of Wilson in the nosological arrangement and for the treatment of cutaneous disorders, leave little room to complain of the hollow jargon and bluster of high sounding words as applied to this branch of medical science. Nor will there any

longer be a reason for the neglect of their study on the part of the young man who is preparing himself for the practical service of the physician and surgeon, either in his own consulting room, or at the bedside of the patient. Nor need the limber tongue of the professor, who should be regarded as a reliable oracle in the department in which he instructs, again be heard to designate as *tinea capitis* a case of impetigo, which happens to be located on the face, and which is on exhibition before a medical class. This blunder, incredible and reprehensible as it may seem, we know to have been made not five years ago, in one of our high courts of medical erudition and science, without a blush or without correction from the gentleman who committed it. As we speak no names, either of parties or of places, we allude to the *fact* without apology. We have to remark, in this connection, that had the profession been on terms of greater intimacy with Wilson, such a diagnosis would not have been palmed off upon scores of young students, a portion of whom were unable to reconcile what they heard with what they saw. Such ignorance, at the present day, is without excuse, although in the early dawn of medical knowledge it might have been winked at. The great work of Wilson will be an important help, a reliable text-book for all students and professors and practitioners, by whom it can be profitably consulted in doubtful emergencies, both in the diagnosis and treatment of individual cases. We have heard Wilson denounced by men holding a high position in the medical ranks, as being an obscure writer, using language which they could not comprehend. These objections, however, have uniformly been of that class of practitioners who are totally unfamiliar with the first elements of Dermatology; and who, although excellent in the ordinary calling of physician or surgeon, find themselves in a thick wilderness whenever cutaneous pathology or cutaneous therapeutics is the theme of conversation. To be thoroughly understood, Wilson needs to be studied with the same thoughtful attention and concentration of the mind required for a proper comprehension of Homer or Horace in the ancient classics, Lord Coke or Chancellor Kent in the law, Velpeau in surgery, or Mason Good in the general science of medicine.

The derma and its related dependencies, its multitudinous glands, and their follicles or tubes—all highly complex in organization, and performing a diversity of functions essential to the well-being of the total physical economy, are subject to various abnormal conditions resulting from *inflammation*. These conditions admit of being divided into two primary groups, namely—

I. Diseases affecting the general structure; and,

II. Diseases affecting the special structure of the skin.

Under the first primary group of diseases affecting the general structure, the *Etiological* system of Wilson embraces seven typical forms, five of which correspond with as many orders of the Willanean classification, namely, Exanthemata, Papulæ, Vesiculæ, Pustulæ, and Bullæ. These our author regards as arising from *general* causes. The term general, as employed in this connection, is intended to suggest the idea of a cause originating in the mal-performance of the ordinary functions of animal life, rather than to include a variety of causes. Wilson holds to the opinion, that all the diseases included under this head proceed from one primary and essential cause—that is, mal-assimilation; in other words, from an irritant poison generated in the organism of the individual as a consequence of mal-assimilation—the morbid action in the cutaneous tissues being a vital process, having for its object the elimination of that poison by the skin. It has been a dominant notion with many practitioners of sound sense, that in diseases of the skin there is present in the system some peccant element, which displays its malign influence upon the cutaneous surface in preference to seizing upon other organs, for the rea-

son that nature instinctively employs this organ as the medium through which she essays to rid herself of this inimical principle, whatever the principle may be; and in performing this work, certain abnormal processes are brought into play. The idea of Wilson is but a clearer, more definite, and more scientific embodiment and expression of this notion. It savours largely of the old humoral pathology of Brown, for so many years regarded as obsolete and with disfavour—as a sort of fossil relic handed down from the dark ages of medicine. But, after all the vicissitudes and fortunes which it has encountered, this same Brunonian pathology is once more considered to have its foundation on the rock of truth, and not in the crude speculations of the cloister, or in the blind credulity of ignorance. And the medical profession, as well as those who may hereafter require their services for the amelioration or cure of cutaneous disorders, will owe to Wilson a debt of gratitude for the lucid manner in which he has presented this branch of his profound and difficult labours; for these labours will conduct to the most auspicious results in the field of therapeutics.

Admitting that mal-assimilation is the great primary cause of diseases affecting the general structure of the skin, the *variety* which is presented in the form and condition of the apparent disease, is simply the product of certain phenomena that are known to modify the features of disease when *any other organs* of the system chance to be the seat of that disease; the result, in fact, of predisposing causes, either congenital or acquired.

Diseases arising from general causes are divided and grouped in the following manner:—

a. Erythema (exanthemata).
Pityriasis.
Erysipelas.
Roseola.
Urticaria.
b. Lichen (papulæ).
Rosacea.
Strophulus,
Prurigo.
c. Eczema (vesiculæ).
Psoriasis.
Sudamina.
d. Impetigo (pustulæ).
Ecthyma.
e. Herpes (bullæ).
Pemphigus.
f. Furunculus,
Anthrax.
g. Purpura.

To those who are accustomed to the study of cutaneous pathology, according to the nomenclature of Willan, or according to the previous editions of Wilson, it may seem not a little odd, that psoriasis should be transferred from the position it has so long held among the squamæ, to be introduced into the order vesiculæ. We confess we should as soon thought of looking for a tropical plant in the frigid zone; or we should as soon have expected to find erythema transplanted from the prominent position where it now stands as the representative and type of the exanthemata, to be arranged with the order papulæ or tuberculata. Perhaps time and further study may prepare and accommodate our mind for the unexpected change—a change to which we are not now reconciled. We have already signified our high respect for the erudition of Wilson; and were it our custom to yield in blind acquiescence to any writer on the subject of dermatology, Wilson would be that man.

In chronic cases of psoriasis, or psoriasis inveterata, a trifling, serous exudation, of entirely insignificant import, is occasionally seen in the fissures produced in consequence of the long existing hypertrophy and low degree of inflammation; but a retrospection of the eruption conducts back to a small, hard, red papule, which, after an existence of two or three days, has its summit surmounted and whitened with a minute scale; and if its initiatory pathological change is suggestive of any change in name, it would be, in our opinion, that instead of a squama, psoriasis might, with some seeming pro-

priety, receive the cognomen of papule, and be affianced in the same order with lichen. But when we find psoriasis coupled in the same order with eczema—a disease characterized from the very outset by the formation of almost innumerable vesicles—colourless and transparent globules, we confess our inability to harmonize the elements of the latter affection, with those small, solid, scaly elevations, which announce the disease to be psoriasis. Whatever changes or varying products may characterize either of the disorders here alluded to, during their more advanced periods, no alteration in the generic name, suggested by the typical form of these maladies, would be called for in consequence of these ulterior changes. Time operates great transformations in diseases as well as in other matters—in men—in trees—in flowers—in rocks. Take, for instance eczema. To day, it may be purely a vesicular eruption. To morrow the vesicles will have risen perhaps to imperfect pustules; and the diseased patch will yield a sero-purulent discharge, and will present other features, which will bring it into near alliance with impetigo. Nothing, in fact, is more common, than some such modification of the original, typical form which the eruption presented. Where such transformation takes place, the dermatologist adds a qualifying term. In the instance supposed, the name eczema receives the appendix *impetiginodes*. This covers the whole ground. The nosologist and pathologist are both satisfied and agreed. Thus in psoriasis: If a case happens, during the existence of which a slight and occasional ichorous exudation constitutes one of its features, perhaps the term *eczematous psoriasis* might be admissible; although it is extremely doubtful if the vesicles of eczema are ever detected in psoriasis. We have examined hundreds of cases of the latter complaint, and in none did we ever discover a vesicle, such as presents itself in eczema, or anything akin to it. And we would ask, who has? In regard to eczema, it may be remarked, that however chronic it may be, it never loses its tendency ever and anon to develop vesicles. These vesicles, in aged or very feeble subjects, sometimes part with their watery contents by the process of absorption; and the cuticular capsules remain unbroken for a considerable time; are a mere scale, covering the derma; and to the unpracticed eye bear a strong resemblance to psoriasis. And we have seen in individual cases, where a brief contest seemed to have sprung up between the two maladies now in question—the one apparently struggling to supplant the other; but being of such opposite elementary attributes, and endowed with such different natural tendencies—the one to generate the vesicles, and the other not, we are never at a loss to draw the line of demarcation.

In his seventh chapter, and in connection with what is offered relative to eczematous eruptions, Mr. Wilson has the following explanation in reference to the removal of psoriasis to the position it now holds among the vesiculæ:—

"In the present group of cutaneous affections, the eczematous group, I have thought it right to restore to its proper place the term derived from the Hebrew, tsorat, namely, *psoriasis*, which, as Mason Good observes, 'having lost its primitive and restricted signification, *seems to have wandered in search of a meaning*, and had at different times, and by different persons, various meanings attributed to it; being sometimes used to express scaly eruptions generally, sometimes the scales of leprosy; but last, and with a pretty common consent, the far higher efflorescence of scaly tetters or scalls, denominated in the Levitical code, *saphat;* and by the Latins, *scabies*, or impetigo sicca.' Psoriasis, in its proper acceptation, signifies a scaliness of the skin, resulting from chronic erythema, attended with thickening of the tissues of the derma, and more or less chapping of the inflamed part; in a word, *chronic eczema*, when eczema has produced a thickened and chapped state of the skin, and ceasing to pour out an ichorous secretion, throws off from the inflamed surface a suc-

cession of scales; or *chronic lichen agrius*, when lichen has left a similar condition of the skin, the papules of the original eruption being obliterated in the general thickening of the chronically inflamed part. In truth, psoriasis is to eczema and lichen agrius, what pityriasis is to erythema.

"It will be seen by these observations, that I might have introduced psoriasis into the group of lichenous eruptions, following lichen agrius; but I have preferred to attach it to eczema, because, in truth, lichen agrius and eczema, as being in part a lichen passing into the stage of eczema, or assuming the characters of eczema, of being indeed a lichen eczematosus."

The second sub-group or division, embraces those diseases which arise from special external causes—such as parasites in or under the skin; and those diseases which are produced by the destructive agency of heat and cold. Of the maladies affecting the general structure of the skin, it is in this group alone that we meet with affections of the cutaneous surface, independent of constitutional origin;—those of other groups being essentially *blood-diseases*, and arising from a morbid condition of that fluid.

The third group, which is an important one, includes diseases arising from special internal causes. The number of these diseases is five—lepra, a squama; and lupus, scrofuloderma, kelis, elephantiasis, the last four being of the order tuberculata of Willan. In this group the cause is unknown, or merely hypothetical.

The fourth group relates to diseases arising from the syphilitic poison. This cause is well known and universally recognized.

The fifth and last group, embraces those diseases which arise from animal poisons of unknown origin, and give rise to eruptive fevers. The individual diseases proceeding from these poisons are, rubeola, scarlatina, variola, varicella, vaccinia. All the foregoing groups affect the general structure of the skin; and constitute Wilson's first great division. The *etiological* method of classification is confined to this category of maladies, which, with the exception of the second sub-group, have their source in constitutional disturbance—in some abnormal state of the vital fluid.

The second grand primary division comprises those affections which have their seat in the special structures of the skin; that is, in its vessels and nerves; in its papillæ; in the pigmentary principle; in its glands, hair, and nails. Wilson regards these constituents and appendages of the skin as so many *heads*, under which the morbid affections may be considered. A small number of these diseases are constitutional; but most of them are simply local—pure diseases of the skin, and removed from the influence of the constitutional powers; and therefore the application of the *etiological* nomenclature would not be attended with any advantage over the old physiological system; and to the latter our author adheres in the classification of this second *great division*.

At the end of the chapter, which is devoted to a consideration of the subject of classifying the various disordered conditions of the skin, a *tabular view* is presented, and is divided into two parts, corresponding with, and naturally growing out of, the previous text. That is, the names of the individual diseases affecting the general structure of the skin, are placed before the reader, by themselves;—and lastly, the diseases affecting the special structures of the skin, are named and arranged by themselves;—the particular anatomical constituents of the skin, in which these local maladies exist, are also specified. Thus, the student is furnished with every possible help which he can reasonably desire, or which the nature of the subject renders practicable.

The observations relating to the general pathology of the skin are highly interesting and important. The clear and logical manner in which the sub-

ject is presented, is entirely satisfactory, and is eminently calculated to instruct. It is impracticable, in this connection, and in justice to the author, to offer an epitome of what he has written with so much care and good sense, with a view to show the analogy between different diseases of the skin. We choose to commend the entire chapter to the careful attention of all who are either practically or theoretically interested in the subject of Dermatology. While it is with unfeigned pleasure that we speak thus of the great merits of this chapter, we feel justified in saying that it contains a remark in regard to Lichen, which is at variance with our own observations; and we doubt not but Mr. Wilson will listen candidly to the few words we take occasion to submit upon the point we refer to. We are told that

"The papules of lichen are never found upon the scalp, the palms of the hands, or soles of the feet; in the former situation, probably because the pores are large, more firmly retained in their place from their relations to hairs, and the skin between the pores more susceptible of congestive action. In the latter they do not occur in consequence of the thickness of the cuticle."

A few years since a boy, eight years old, was under our care for a chronic lichen, which had been upon him for more than two years. The papules were disseminated over large districts of integument covering the body, limbs, face, and *scalp*. While we had charge of the case it was seen by several medical friends. Within the past year, a woman forty years of age, of thin, spare habit—a great sufferer from dyspepsia—consulted us for a papular eruption on the hands and lower part of the forearms. The palmar and dorsal aspects were about equally occupied by the eruption, which was a well developed lichen. It lasted about four months before it wholly disappeared.

The lad above mentioned was cured during a voyage to the Sandwich Islands, the place of his birth. In the case of the woman, all the fingers and both thumbs were more or less involved in the eruption, as well upon the inner as outer surface. The two instances here cited go to show that one man, although occupying a favourable position for observation, may not see everything that appertains to the most common diseases, whether superficial or otherwise.

The general therapeutics of the skin constitute a very essential and prominent feature of the volume before us. They are considered in relation to cutaneous diseases that are acute and chronic: in reference to the three periods of life, infancy, manhood, and old age; in respect to different temperaments, normal and abnormal; in persons of different idiosyncrasy and diathesis; and in different conditions of vigor or debility of the system. The local and constitutional methods of treating cutaneous affections, in all their various stages, whether of an acute or chronic form, are thoroughly discussed; and the value of each method is presented to our consideration. The two branches of the subject, that is, the medical and surgical, are harmoniously blended together in a manner that will best secure that mutual aid, which is so important and efficient in carrying out a scientific and successful management of these diseases. A union of the qualities of a well educated physician and surgeon is required. The services of neither can be discarded or dispensed with. The etiological principle of classification, adopted so far as relates to those diseases that are believed to have their origin in an unhealthy condition of the blood, naturally suggests and develops this view of the mutual advantages and importance of having the surgical and medical features of the treatment nicely balanced. The mere local disease—the eruption of the skin, is but a symptom, an index of the disorder of the blood. To one familiar with the cutaneous eruption that may be present in any individual case, the

cause of the constitutional aberration will be apparent. The external symptom reveals the diathesis of the patient, and gives a clue to his medical history; and to the intelligent mind of the practitioner, who understands the cardinal principles of surgery as well as those of the medical art, this same abnormal state of the skin—this local symptom spread out for visual inspection, will be suggestive of a proper line of conduct in attempting to remove the constitutional disorder, as well as that which is displayed upon the skin.

Diseases of the skin, issuing from mal-assimilation, are rarely benefited, our author tells us, by depletion either with the lancet or leeches. This doctrine is worthy of all acceptation. Years ago this was our creed; we can hardly say from whence we received it; but we have long practised upon it. The best remedies, even in the *acute* stage, are moderate purgativss, effervescent salines, diaphoretics and diuretics; and when the acute stage has passed, Wilson advises tonics, bitters with mineral acids, or with alkalies—with or without the ferruginous salts, according to the hæmic or anæmic condition of the patient.

In *chronic* affections of the skin, the presence and the effects of mal-assimilation are more or less obvious; and are evinced by various symptoms, independent of any cutaneous derangement. A careful examination will bring these symptoms to light. Although the patient may consider that he is in fair health, it will be found that the functions of digestion are badly performed. There is mal-conversion, mal-appropriation of the food taken to support the system; there is imperfect chyme and imperfect chyle, and of course impure blood; and the organs, which it supplies, are injured in their physical condition, and impaired in their functional powers. The muscular apparatus is enfeebled—the heart palpitates; the brain and nervous organization suffer—the intellect is depressed, and other mental derangements ensue. Dyspepsia may be a trifling affair in itself, and mal-assimilation a small matter when considered apart, but Wilson regards these conditions as the authors of various cutaneous maladies; and when thus viewed in relation to their consequences, they may very properly be ranked among the most serious of the bodily infirmities of man. The current of impure blood goes into the minutest tissue and into the most attenuated ramifications of that tissue, throughout the corporeal frame. The lungs, the liver, the kidneys, the skin, are all irrigated and enfeebled by this poisonous stream. The whole system becomes loaded with morbid humours of greater or less intensity; and the skin, as one of the emunctory channels, is called upon to eliminate the disturbing agent, while the other emunctories may be left to perform their normal service. The enemy is driven to the surface, causing congestion; and the extra labour of the cutaneous vessels commences. The result is, that the patient may have an eruption of erythema, a lichen or eczema. Here we have an illustrative example of the humoral pathology and the faith of our author in its reality explicitly announced. He holds and inculcates the same views in relation to diseases more profoundly located.

"What," he asks, "determines the seat of the congestion? To which the answer is plain—Temperature, season, age, idiosyncrasy, previous debility or disease, the predisposing or exciting cause. In the cold seasons of the year, the morbid humours, driven, for elimination, upon the mucous membranes of the lungs, may occasion bronchitis; in warm weather they may be precipitated on the mucous membrane of the alimentary canal, causing diarrhœa; under the influence of moral excitement, the result may be apoplexy; or, in other states of the system, gout, rheumatism, or neuralgia. Or, in the absence of, or even in association with any of these, the seat of elimination may be the skin, &c."

It has already been remarked that the methods of treatment, as relates

both to acute and chronic cutaneous disorders, are in conformity with the pathological ideas, which are so eloquently urged upon the practitioner.

If the symptoms assume an acute, inflammatory type, the febrile excitement is to be reduced, and an attempt must be made to neutralize and eliminate the morbid properties of the blood. In chronic diseases all this must be done, while at the same time the constitution must be buoyed up, by resorting to a tonic course of remedial measures. In some patients a depression of the vital energies is produced by a mere accumulation of morbid humours, in which case, our author argues, simple elimination by purgatives will strengthen the whole frame, and will heighten, rather than diminish, the power of the patient. Such cases, however, are rare, compared with those which, from the beginning, require a tonic system of treatment, rather than cathartics.

When a patient, suffering from chronic cutaneous disease, presents himself for medical advice, the practitioner should set himself to inquire into the particulars of the case, and then propound to himself: what are the indications to be fulfilled? They are, *firstly*, to eliminate; *secondly*, to restore power; *thirdly*, to alleviate the local distress. These three indications are considered in detail, together with the means at the command of the medical adviser, for fulfilling them. This portion of the volume supplies much valuable instruction, not met with in any other work; and it will be found of great utility in the diffusion of sound philosophical principles, relating to the treatment of all classes of patients on whom any of the proteiform varieties of cutaneous affections may exist. The author draws largely—almost exclusively—from the rich stores of his own scientific researches and his practical experience; and there is an unction of originality, blended with profound and enlarged erudition, which gives a refreshing charm to the work, from beginning to end. No mere speculative views are allowed a place in this volume, which, without a doubt, will for a very long period, be acknowledged as the chief standard work on Dermatology. The principles of an enlightened and rational therapeia are introduced on every appropriate occasion. The general practitioner and surgeon who, peradventure, may have for years regarded cutaneous maladies as scarcely worthy their attention, because, forsooth, they are not fatal in their tendency; or who, if they have attempted their cure, have followed the blind guidance of empiricism, will almost assuredly be roused to a new and becoming interest in this department of practice, through the inspiring agency of this book. We speak not extravagantly when we say that the learning which pervades and characterizes this treatise, is so agreeably presented, that no medical reader having any claim to scholarship, or having any abiding desire for improvement and for professional usefulness, can fail to reap a large benefit for himself and for his patients, by a close study of its doctrines, and a careful observance of its practical directions and suggestions in matters appertaining to the general and topical management of diseases of the skin. The untiring zeal and commanding talents of Wilson will draw many disciples after him. This is as it should be. They will find him no uncertain, flickering taper; but a clear and brilliant light, shining with steady radiance, and guiding their steps in safety. And they will also learn, if they do not already understand, that no man can be a skilful practitioner in Dermatology, unless he is something more than a mere specialist. He must be endowed with enlarged views and varied knowledge and sound judgment in the several branches of learning that constitute a thoroughly educated physician and surgeon.

S. D.

BIBLIOGRAPHICAL NOTICES.

ART. XVII.—*Transactions of State Medical Societies.*

1. *Transactions of the Medical Society of the State of New York, transmitted to the Legislature, Feb.* 9, 1857. 8vo. pp. 292.
2. *Proceedings of the Sixty-Fourth Annual Convention of the Connecticut Medical Society, May,* 1857. 8vo. pp. 71.
3. *Transactions of the Eighth Annual Meeting of the Medical Society of the State of North Carolina. Held at Edenton, N. C., April,* 1857. Wilmington, N. C., 1857, pp. 90, 8vo.
4. *Transactions of the Medical Society of the State of Pennsylvania at its Annual Session, held in West Chester, Chester Co., May,* 1857. 8vo. pp. 218.

1. THE several addresses, biographical sketches, and professional papers, embraced in the *Transactions* of the New York State Medical Society, at its session of February, 1857, are of a highly interesting character. Nearly all of the latter are replete with instruction, presenting the results of direct personal observation, or hints eminently suggestive on subjects of deep importance to the medical practitioner.

The semi-annual address, delivered before the State Society by Dr. Alden March, presents a rapid sketch of the progress, in this country, of agriculture, commerce, the arts and sciences generally, during the last fifty years, in comparison with the advancement, during the same period, of every department of medicine, whether considered as a science or an art.

The semi-annual address before the Medical Society of the County of Albany, by Dr. Sylvester D. Millard, is devoted to a brief historical sketch of the Society, with notices of some of those who have been its prominent and most efficient members during the last half century, but have passed from time to eternity.

Both of these addresses are filled with matter that cannot fail to interest and instruct all who feel a pride in the onward march of our profession, and its increased facilities for the accomplishment of its mission of benevolence, or are curious to learn something of the characters and career of those who, in times gone by, constituted the leading members of the profession in one of the most ancient cities of our Union.

To these addresses succeed biographical sketches of Drs. Moses Hale, John McClellan, and Henry Mitchell, deceased members of the New York State Medical Society.

The first of the strictly professional papers is on cholera infantum, diarrhœa, and entero-colitis—their relation to each other, and their treatment—by Dr. Edward Parker of New York City.

Dr. Parker examines each of the three following propositions, being those generally received by the profession, or, at least, generally laid down as proved by our systematic writers on the subject:—

"1st. Cholera infantum is a disease peculiar to this country, though not entirely unknown in Europe.

"2d. Cholera infantum is almost entirely confined to large cities, and is rarely seen in the country.

"3d. The symptoms, course, and pathology of the disease, entitle it to a separate place in our nosological tables.

"4th. The treatment is to be distinct from that of diarrhœa, on the one hand, and of dysentery on the other.

"5th. Perhaps the most alarming symptoms are those of hydrocephalus, occurring in the advanced stages."

The correctness of all the foregoing propositions is denied by Dr. P. He

adduces the descriptions given by Dr. West, of London, of inflammatory diarrhœa, and by Dr. Bouchut, of Paris, of infantile entero-colitis, which, in all their essential features, will certainly represent, very accurately, a well marked case of cholera infantum. There can be no doubt that a disease identical with that known among us as cholera infantum, does prevail, to some extent, in different portions of Europe. It is nevertheless true, that infantine cholera is the especial endemic, during the hot season, of the larger cities of our Middle, Southern, and Eastern States, and that nowhere else does it prevail to the same extent, and is productive of an equal amount of mortality.

That cholera infantum is in a great measure confined to our larger cities, and to the most unhealthy, confined, and overcrowded localities within these, is a fact so clearly demonstrated by a series of reliable mortuary statistics, that we were somewhat surprised to find it so pointedly denied by Dr. P. The evidence he has adduced to sustain his denial, is particularly loose and inconclusive. No dependence is to be placed upon the census returns quoted by him, as, in the year to which they belong, instead of only 260 deaths occurring in the entire State of Pennsylvania, the reports of the Board of Health show a total of 505 for Philadelphia alone. But we do not deny that the disease may occur out of cities. Wherever children, about the period of their first dentition, are exposed to an intense heat, and, at the same time, to an impure, damp, and stagnant atmosphere, and are badly fed, and lodged at night in crowded, filthy, and ill-ventilated apartments, whether in or out of the city, they will be liable to an attack of cholera infantum; but as this concurrence of morbific agents is more commonly met with in the narrow courts and alleys, and the badly constructed and overpopulated dwellings of the poor, in large cities, it is there that the disease prevails every summer, and is productive of the greatest mortality.

We have no disposition to deny the correctness of Dr. P.'s conclusions in regard to the third proposition as stated above. Cholera infantum is unquestionably only an aggravated form of infantile diarrhœa; it is admitted, on all hands, to be, in its second stage at least, essentially an entero-colitis, produced by the particular morbific influences already referred to. This is proved beyond the possibility of doubt by the pathological researches of Dr. Edward Hallowell, of this city. (See this Journal for July, 1847, p. 40, et seq.)

Although, in its first stage, cholera infantum consists simply in an increased and somewhat altered secretion from the mucous follicles of the intestines, yet as this is very speedily succeeded by more or less extensive inflammation, we agree entirely with Dr. P. that the general indications for the treatment of the disease are the same as in diarrhœa and entero-colitis; recollecting, however, that as the main agents in the production of cholera infantum are intense and continued solar heat, a damp, confined, and vitiated atmosphere, and improper food, regular and continued exposure of the patient to cool, dry, fresh air, and a well regulated, wholesome diet, are of primary importance to insure a speedy cure: with these, and daily bathing in warm water, a very simple course of positive medication will be demanded; without these hygienic measures, all other remedies, however well selected or assiduously administered, will fail to produce any permanent good.

There can be no doubt that, in very many cases of cholera infantum, the cerebral symptoms which are attendant upon the advanced stages of protracted cases, are the result of exhaustion and anæmia, and not of meningeal inflammation. We have, nevertheless, had occasion, in numerous instances, to observe, at a somewhat early period of the attack, all the symptoms of acute meningitis to present themselves, and terminate promptly in effusion. That in these cases, the cerebral symptoms were the result of meningeal inflammation, was shown by repeated examinations after death.

A very sensible and opportune paper on "Improvements of the Public Health, and the Establishment of a Sanitary Police in the City of New York," is from the able pen of Dr. John H. Griscom.

Public hygiene, though calculated, when systematically and rigidly enforced, to promote the health, comfort, and pecuniary interests of every community—to eradicate most, if not all, endemic diseases that prevail in their midst, and

to protect, to a very great extent, against the incursion of even the most fatal and dreaded epidemics, or, when these make their appearance, to disarm them of much of their malignancy, and reduce the number of their victims—is scarcely recognized as a legitimate subject for legislative or executive action.

"The great and serious fact," very pertinently remarks Dr. G., "seems scarcely yet to have penetrated very deeply into the spirit of government and legislation, that there is *such a thing as sanitary science;* that the object of this science is the ascertainment of the laws of health and disease, and of the circumstances which promote and control them; that though comparatively in its infancy, and demanding for its advancement all the time and talent we can bring to bear upon it, it is still so far understood as to enable us to make a practical application of its principles to the prevention of disease, and the preservation and prolongation of life; and here let me say, that it is not in the light of epidemics alone that this science is properly received; in the truthful language of a contemporary writer, the public are too apt to 'lose sight of the less violent, slower, but no less certain causes of sickness and death with which they are continually surrounded. Death reaps a yearly harvest to the grave, and when it exceeds not the usual number it is not heeded, and the inquiry is not made whether a part of his trophies might not have been snatched from him.'[1] While it is the direct purpose of sanitary science, and the legislation which should flow from it, to reduce the draughts of death by preventing epidemics not only, but all the minor disorders also by which he gathers us in.

"Sanitary science, while directly allied to, and forming a part of the great science of medicine, is in a measure distinguished from it in its peculiar relations to mankind, and in the application of its laws for their relief. Both require a thorough knowledge of physiology, or the laws of life; of practical chemistry; of the symptoms, causes, and treatment of disease, and of pathology, or the laws of morbid action. But for the sanitarian physician, superinduced upon these, there is especially requisite a knowledge of *forensic medicine,* or the relations of the science of medicine to law; of meteorology, or the effects of climate and atmospheric influences on the body; of the physical character or dynamics of the atmosphere; of the philosophy and practice of ventilation; of various matters of a mechanical kind bearing on sewerage, house building, street cleaning, and water supply; of statistics of life and mortality; of the literature of epidemics, and of all sanitary improvements. And, lastly, to be a good sanitarian, requires the possession of sound logical faculties; the capability of dealing with facts and opinions without mistaking coincidences for causes, or building up great theories on insufficient data, or from great facts deducing absurd conclusions. Thus it will be seen that one may be a good practising physician without being a sanitarian, but no one can be a good sanitarian without being a good physician; the requisites for one underlie those for the other, as a foundation."

"The very first duty which should be required of an officer of health is, *vigilance in the discovery of the actual existence of disease.* He must be a detective policeman, and must therefore be familiar with the appearance of disease of every kind; as upon his diagnosis may depend the loss or salvation of many lives, and large amounts of property. Who but a well educated physician, therefore, is competent for this *first* of all sanitary duties?

"Presupposing this qualification, let us glance further at his duties; they may be stated as follows: to ascertain and make reports as to the sources of disease, or the circumstances which promote their diffusion among the population; to define the districts in which they spread; to ascertain the character of the streets and houses in such districts; to describe the nature and extent of the internal accommodations of the dwellings of the labouring classes, how much they may be overcrowded, and of their means of securing cleanliness, removing filth, promoting ventilation, and providing warmth with economy; to discriminate between the direct influence upon their sanitary condition of the habits of the poorer classes and the external circumstances by which they are surrounded, while avoiding the error of supposing those habits to be inde-

[1] Newman, Report on Sanitary Police.

pendent of other domestic arrangements; to ascertain whether any and what injurious consequences are clearly attributable to certain manufactures; to the location of slaughter-houses, tanneries, burying-grounds, &c., amidst dense populations; to distinguish the extent of disease caused by removable agencies; by malaria from defective drainage, by bad construction of dwellings; and by destitution of the means of subsistence; to ascertain from personal observation the condition of the soil, its geological nature, and power of absorbing and retaining moisture; the extent of the supply of water for public purposes and domestic uses; and the quality and extent of the sewerage; to advise and recommend measures for purification and improvements in all these matters; to ascertain the existence of *particular* diseases which increase the ordinary rate of mortality, as well as keep thoroughly informed as to the general and relative mortality of the district; in a word, to have a perfect knowledge of the sanitary state of the inhabitants, and to keep a watchful eye upon all its changes, so that a perfect system being introduced, the routine of daily labour and daily cleansing should prevent any accumulation of noxious substances, and render unnecessary any cleansing works of magnitude to meet extraordinary visitations of disease. Connected with which duties would be the inspection of all public buildings, churches, poor-houses, hospitals, asylums, lodging-houses, tenant-houses, so called public and private schools, and charitable institutions; the pointing out of means of insuring adequate supplies of pure air, and of exit for that which is vitiated; and the keeping of records of meteorological changes, and studying their relation to the appearance and progress of disease.

"To sum up the whole, of which the above is but a partial enumeration, an officer of health should be duly qualified and required to act as the efficient medium by which all the circumstances which affect the health of man—and their name is legion—may be duly considered and weighed; to classify and arrange them, that they may be brought under simple heads, and consequently be more easily remedied either by voluntary efforts or legislative enactments, so that all classes, both rich and poor, may be relieved from the cruel alternative of being compelled to submit to deleterious and destructive influences or of being turned out of their dwellings, which, though unwholesome, are still their *homes*. We need scarcely add, that the sanitary department of every city should be empowered by law to enforce all measures calculated to promote these important results.

"There are yet two other points in which the services of properly qualified and authorized sanitary officers might be of invaluable benefit to the people, especially the poor; who we now know suffer immeasurably in purse and health, and are without remedy. These are the detection and exposure of *adulterations* and impurities of *food and medicine*. The extent to which these fraudulent operations are carried is scarcely credible, and cannot be understood except by the initiated."

"Having fixed in our minds this theoretical view of what should be accomplished with a corps of properly qualified functionaries, let us next see the materials for such a force; whether it is possible to find men of the required capacity and intelligence, and whether their services are obtainable, a brief statement of facts confined to the city of New York, but applicable elsewhere, will satisfy both inquiries.

"Besides the existing department, which is *supposed* to have a supervision over sanitary matters, we have another institution devoted to the same general object, viz: the protection of the health of the people, in the service of which, supported partly by public and partly by private bounty, are employed in its out-door duties a staff of some fifteen active and intelligent medical men. The institution here referred to is the *dispensary* system of administering medical aid to the indigent. The five dispensaries cover nearly the whole city. The amount of good work performed by the physicians of these institutions cannot be expressed in figures, but some idea of it may be obtained from the fact that they administer professional relief annually, at their dwellings, to about 22,500 persons, making in one year, at the lowest estimate, 67,500 domiciliary visits to these destitute people, living as they almost invariably do in cellars,

attics, courts, alleys, and crowded tenements, in the most degraded and filthy portions of the city." "Here, then, we have a body of men not only continually cognizant of the diseases which threaten the people's lives, and in contention with those diseases to preserve those lives, but more important still they have a personal knowledge of their sources, and are personally and deeply interested in their abatement. Their duties call them to the very localities in which pestilential diseases first make their appearance; they are the first to recognize their existence, it is their duty to combat them; and theirs should be also the power to exert all the force which science and law can furnish to check the evils in the bud.

"Thus is the question answered whether there can be found men capable and willing to act the part demanded of a model sanitary officer. For half a century just such a body of men has been quietly and unostentatiously performing this great work of humanity, and all the valuable aid which the city might have derived from them, in addition, as public officers of health, has been overlooked and despised. There cannot be a question that, had the two systems of preventive sanitary police, and dispensary medical aid been united in one, under an energetic administration, during the period of their mutual existence, the expenses of both would have been many, many times saved, together with innumerable lives. But, instead of this, while the dispensaries, which are private institutions, though performing a most important public function, have advanced in usefulness and dignity with rapid strides, the public health department has degenerated almost as rapidly as the other has risen, until at last it has become a by-word and reproach among all classes of citizens. Of the causes of this we need say no more, in addition to what has already been hinted, than that it is due in great measure to the absurd practice of mingling science and humanity with politics; as rational would it be to choose our dispensary and hospital physicians by general election from among the mercantile and mechanic classes, as to select our public guardians of health from those sources, and in such a manner as is now done."

On the types of fever, by Dr. George Burr, of Binghampton, is the title of the next paper.

Dr. Burr defines the term *type* as applied to fever, to signify a peculiar form or character of the disease which distinguishes it from other varieties of fever, and which, when correctly determined, gives us a more correct and definite idea of the nature of each particular case that comes under observation. Every form of fever possesses certain symptoms common to all, which have been received as the especial febrile symptoms; now these symptoms may be so grouped, or exhibit such peculiarities either in violence, duration, regularity, location, &c., as to indicate unmistakably a radical difference in the nature and efficient cause, in the various forms of fever which are, from time to time, to be observed. These several groupings or classifications of febrile manifestations enable us to distinguish one form of fever from another, or in other words determine its type.

With this very loose and unsatisfactory definition of type, Dr. B. proceeds to consider the malarial or intermittent type of fever, including bilious remittent, congestive, and pernicious fevers, and the yellow fever, which he considers a distinct type. The continued type of fever he reserves for a future communication.

The remarks of Dr. B. on the symptomatology, pathology and causes of malarial fever, and his concise notice of yellow fever, are in the main correct and pertinent; we find in them nothing, however, particularly novel or striking.

It is a general opinion among American physicians that periodical fever is less prevalent of late years in what have been considered as well-marked malarial regions, and that fever of a typhoid character now predominates in many of those districts of country of which intermittents and remittents were formerly the especial endemic. From this opinion Dr. B. dissents. He believes that the fever now recognized as typhoid by many of our physicians, is in its type essentially malarial, and for the following reasons:—

"1st. It prevails in the latter part of the summer and during the autumnal

months more generally—sometimes in the spring, corresponding to what are termed vernal intermittents. It is often accompanied by dysenteric symptoms, and in that form sometimes resembles an epidemic.

"2d. Bilious symptoms predominate. The tongue is usually coated heavily, there is more or less nausea, and the stomach often loaded with vitiated bile, and the invasion of the disease is marked by rigors, followed by febrile reaction, with, in most instances, distinct remissions, during which the skin is bathed in perspiration; at other times the fever seems more continuous, and the period of remission hardly perceptible. This is more particularly the case where the force of the attack is directed against some particular organ, as the brain, lungs, or gastro-enteric membrane, &c.

"3d. These cases are best treated by the early administration of quinine, given during a remission of the febrile symptoms. My own observation, and the experience of others in a somewhat extended acquaintance, justify me in making this point. When given at the proper stage, the whole febrile movement becomes modified, and in due time disappears, without that prolonged condition which some have called the typhoid stage of the disease.

"It is not intended to deny the existence among us of that form of continued fever denominated, by Louis, typhoid fever. It is, I believe, however, very rare, and is to be met with under very different circumstances from those which accompany the form of fever we have been regarding."

A case of death by syncope, from plugging of the pulmonary artery, reported by Dr. O. M. Allaben, is in many points of view a highly interesting one. It will be noticed in another department of this No.

Dr. W. S. Norton, of Fort Edward, relates a case of gangrenous erysipelas, involving the whole of the right hand and arm to the elbow-joint. It occurred in a young lady, twenty-two years of age, and was treated by free longitudinal incisions through the gangrenous parts; the subsequent application of a strong "terebinthinated tincture of cantharides," over the entire arm, followed by poultices, thirty grains of calomel, with five of opium internally, and the free use of brandy punch.

Under this treatment, the case progressed favourably, and at the end of one month from the commencement of the attack, the patient was entirely well, with the exception of an inability to flex and extend freely the fingers of the right hand.

The treatment pursued in this case was, under the circumstances, judicious; we do not consider it of so novel a character as Dr. N. would seem to infer.

A case is detailed by Dr. Nelson Winton, of Havana, in which an ovarian tumour was successfully removed by extensive abdominal section, with recovery of the patient.

A case of medullary sarcoma in the knee-joint, in a girl twelve years of age, in which amputation of the limb was resorted to, is related by U. Potter. It presents no points of especial interest.

To these cases succeeds a paper by Dr. D. P. Bissell, presenting some general remarks on the importance of the adoption of effectual means for the extension of full, efficient vaccination to the entire community, as the only sure protection against smallpox; and on the necessity of securing to physicians a constant supply of pure, reliable vaccine matter. The remarks of Dr. B. under both heads, are sound and pertinent.

He suggests that depots of vaccine virus should be established and maintained in some one or more of our principal cities, where physicians may at all times obtain a supply.

Dr. C. V. Barnett reports a case of supra-malleolar amputation in a case of gangrene of both feet from the effects of frost, and appends the following remarks on the relative value of amputation at this spot:—

"To comprehend its full value when compared with amputations at the usual place of election, we have only to take into consideration that the limb is divided at a point where the lower end of both bones has attained its largest size, and where every muscle of the leg has become tendinous, and above all, every tendon takes a ready and firm attachment to the end of the bones. From this it necessarily follows, that in the application of an artificial limb, you have the

whole weight of the body supported by the attachments of these tendons, instead of the small pointed ends of the bones continually jamming into a tender mass of flesh, as in case of amputation in the fleshy portions of the leg."

The following case of hemorrhage from the urethra, is related by Dr. N. C. Husted:—

"Mr. C., aged 42, of medium height, healthy, and robust, of a nervo-sanguineous temperament; never has had any urethral difficulty. Whilst having an intercourse, he was seized with severe pain about midway in the penis, as he described it; he arose, and found the blood flowing in a full stream from his penis. He laid down again, supposing the hemorrhage would cease, but it did not. He became faint, and sent for me. I prescribed cold to the penis and perineum, and, internally, tinct. ferri muriat., in doses of ten drops every hour, until the hemorrhage ceased. It subsided soon, and in a week he was able to go to his work. Nothing of the kind has occurred to him since.

"I find but one case of the kind recorded, which is very similar in every respect; it is reported in the *American Journal of Med. Sciences* for Oct. 1850."

The scientific contents of the volume before us, conclude with the history of an interesting case of chronic tubercular splenitis, resulting in abscess, or entire disorganization of the parenchyma of the gland, communicated by Dr. G. J. Fisher, of Sing Sing. On examination after death, there were discovered hypertrophy and dilatation of the heart; miliary tubercles throughout nearly every portion of the lungs; enlargement and induration of the liver; its substance beset with tubercles from a microscopic size to that of a small pea.

"The spleen was carefully separated from its surrounding attachments, which were old and strong, apparently the result of adhesive peritoneal inflammation; its bulk was about four times the natural size, it was of an oval shape, and, when removed, was discovered to be distinctly fluctuating. On laying it open by a free incision, it was found to consist of a mere sac, the walls of which did not exceed an inch in thickness, though not entirely uniform throughout. This sac contained a liquid or semi-fluid disorganized mass, which appeared to consist of pus, and the broken down or disorganized parenchyma of the spleen; from the centre to the surface of the organ, every stage of the process of disorganization was to be seen from pus to the slightly changed substance of the gland. The remaining organs of the abdomen presented no marked pathological changes, except the deposit of tubercular matter in the layers of the mesentery, and the enlargement of many of its glands."

2. The several addresses, dissertations, and reports, embraced in the proceedings of the annual convention of the Connecticut Medical Society, held May, 1857, are replete with sound principles, noble sentiments, and useful observations, many of them of deep interest to the members of the profession associated with the Connecticut Society, but not of that general character which would press them strongly upon the attention of those beyond its circle.

The annual address by the President, Dr. B. H. Colton, presents a hasty sketch of the history of the Connecticut Medical Society, suggestions for the increase of its efficiency, and for the enlargement of its sphere of usefulness, with an occasional remark on the practice of medicine throughout the State.

In a well written and manly expressed dissertation, by Dr. Benjamin F. Dean, is presented, in brief outline, the history, present position, and prospective attainments of the medical profession.

A dissertation by P. M. Hastings is occupied mainly with very excellent and pertinent remarks on the value of the microscope in our investigation of the structure and functions of the animal organism, and as a means of diagnosis in disease.

From the sanitary report from Hartford by Dr. Gordon W. Russell, we quote the following, which is a portion of the reporter's able remarks on the subject of vaccination generally.

"The propriety of using the scab or crust for vaccination, instead of the lymph taken about the sixth or seventh day, may well be doubted; not but that a good crust, consisting of hardened lymph, will equally communicate the disease, but the difficulties and uncertainty of obtaining a crust consisting of

lymph alone, becomes an objection. Frequently the vesicle is broken and the lymph nearly discharged, or, in consequence of inflammation, the crust consists in part of dried purulent, or semi-purulent matter which may produce a sore, but not always a vaccine vesicle, and which, if not carefully examined on the fifth day, may be thought to be a genuine one. And here I may be allowed to suggest the propriety of testing all cases of primary vaccination; this would afford a sure and perfect guarantee of the protection given by the first. It is attended, I admit, with some trouble, but if the fee is not already remunerative, it should be made so."

"That revaccination is a necessary operation, protecting the system already wholly or partially exposed to variolous infection, is, I believe, generally admitted, though still denied by some physicians. I confess I have rarely seen a perfect vesicle in a secondary vaccination, when there was already a well-defined, punctated scar from the first. I have occasionally seen near approaches to it, varying, however, in the constitutional symptoms, in the appearance of the areola, and of the attendant inflammation; some of the profession, I know, are reported to have seen it frequently, but I have not been so unfortunate, the nearest approach to it that I have seen this season, was in a lady of at least sixty years of age, who had been inoculated with variolous matter in Scotland when a child.

"Of course, the crust that is formed during these revaccinations, should never be used for the purpose of producing the vaccine disease. It might occasionally answer for the purpose, and of course it would do so whenever it is the product of a genuine vesicle, but these instances are so rare that it is safe to lay it down as a rule that it should never be used.

"I am satisfied that variola is upon the increase, and for years to come will continue to increase, until public necessity shall compel a resort to careful, general, and repeated vaccination. The time may not yet have arrived, when by public authority every child shall be vaccinated before it is three months of age, but when this foul disease shall have become more common, every one perceiving the increase of it, then may we expect such a consummation, and that the law will not be a dead letter."

"There is an interesting portion of this subject, itself alone sufficient for a lengthy dissertation, viz., *upon the communicability of disease by vaccination;* this demands careful and thorough investigation, both to satisfy ourselves, and quiet the public feeling, which is strongly inclined to believe in its truth. Without denying the possibility of it, I am still somewhat skeptical as to its frequent occurrence; though cutaneous disease may occur after its performance, and be the indirect result of it, as vaccination often develops any tendency to disease of the skin, which may be present in the system. I can now recall but one instance in which I have witnessed any cutaneous eruption to follow vaccination this season: this was eczematous in its character, and appeared about the tenth or twelfth day of the operation, behind the ears and upon the lower part of the scalp. The child was teething at the time, which was enough, in my opinion, to account for it; and that it was owing to this, and not to the vaccine infection, was shown by the same matter being used in quite a number of instances, before and afterwards, without any such result being produced. If physicians would make a careful record of all such cases that come under their notice, a mass of information would be obtained, which would enable us to dispel many of the illusions of the public.

"The popular notion that vaccination should be repeated at every certain and defined period of one's life, is altogether wrong; there can, with reason, be no such course marked out; but if the operation was done during infancy, it is proper that it should be tested after the subject has grown up. If anything further than this is done, it must be as much to satisfy the wishes and quiet the fears of the timid, as to comply with any rule indicated by reason or experience."

Besides the papers noticed above, the volume before us contains biographical sketches of Dr. Eli Hall, of East Hartford, and Dr. Sylvester Bulkley, of Rocky Hill. The first of these gentlemen died June 8, 1856, in the 71st year of his age, and the second, February 1, 1856, in the 69th year of his age.

3. The *Transactions* of the Medical Society of the State of North Carolina, at its session of April, 1857, contain many interesting papers.

The first of these is a report on Stomatitis materna, by Dr. Wm. H. McKee, of Wake County.

The disease, Dr. McKee states, has appeared in sporadic cases, in his vicinity, for several years past. Its first symptoms, as observed during the latter period of pregnancy, are an inflamed and extremely sensitive state of the mouth and tongue, rendering salt food and acids intolerable. There is, at the same time, loss of taste, impaired appetite, thirst, with insomnia, or disturbed dreams. In a few days, isolated patches of ash-coloured sores appear on the lips, cheek, and tongue, with a free discharge of saliva. Under proper treatment, all these symptoms soon disappear, but, in most cases, return after parturition; generally towards the end of the second or between that and the close of the fifth week. The soreness of the mouth and tongue is now of a more aggravated grade. The sores are petechial, and soon become confluent, the tongue fissured and swollen, with increased salivation. The patient cannot take sufficient nourishment, the secretion of milk is imperfect, and suckling is attended with pain and great fatigue. The imperfectly nourished child, becomes fretful and restless, and deprives the mother of comfort and sleep. Fever soon sets in.

Dr. McKee remarks that, in every bad case he has met with, there was flooding subsequent to delivery, and a too free discharge of the lochia.

The child, we are told, should now be weaned, and every effort made to improve the condition of the digestive organs of the mother.

After the healing of the ulcers, the mouth and tongue have a glazed and polished surface, and become less sensitive to articles of food. The patient craves the coarsest diet which alone seems to satisfy her appetite. This, after from twelve to sixteen hours, is thrown up either inodorous or decidedly acid. As the disease subsides in the mouth, the stomach and bowels become affected, and there is vomiting, with occasional purging, pain of abdomen, restless nights, emaciation, colliquative sweats; the skin being pale, as it were bloodless, of a pearly whiteness, drawn tightly over the bones, and appearing as if polished; in other cases it has a dark, dry, shrivelled appearance. The abdominal symptoms being partially relieved, the disease shows itself at the verge of the rectum and soon spreads to the vagina, urethra, and womb. The sphincter ani becomes fissured, and a crop of ash-coloured sores surrounded the anus. Defecation is intensely painful, the feces hard and coated with blood and mucus. So severe is the suffering now, that patients have declared they would much rather give birth to a child, than endure a passage by stool. Leucorrhœa of an acrid character attends, and adds to the complication of the disease and the sufferings of the patient. The mouth and fauces become now anew the seat of disease, which extends to the frontal sinuses and facial antri, and, finally, to the lungs, inducing cough, expectoration, pain, night-sweats, and finally dropsical infiltration, succeeded by death.

In regard to treatment. The infant should be weaned if possible. A blue pill followed by a mixture of calcined magnesia and Hoffmann's anodyne, of each one drachm, suspended in sugar and water, will afford great relief. As a local application to the mouth, creasote is one of the best; it may also be given internally with good effect. We may also employ a solution of chloride of soda, decoction of smart weed, sage and alum with honey, olive oil and honey, etc. The internal use of iron is our best remedy; by combining it with iodide of potassium its virtues are much enhanced. The tincture of chloride of iron is a favourite remedy, but the preparation that Dr. McKee has found the best, is the tartrate of iron and potassa, in combination with the iodide of potassium; two drachms of each to eight ounces of Madeira wine and the same quantity of water, taken in the dose of a dessert-spoonful, three times a day—with the free use of morphia at night to control pain and procure sleep; purging with magnesia and an anodyne when required. For the vomiting—the tinct. nux vomica, twenty drops for a dose, in cold water, was generally successful. The patient should take the air whenever able to do so. When there is emaciation, the cod-liver oil may be given with porter, brandy, or whiskey, when the stomach can take it. When the rectum is the seat of

disease, anodyne injections are indispensable. For the leucorrhœa, the solution of acetate of zinc, four grains to an ounce of water, by injection, having first used warm water and castile soap, will arrest promptly the discharge.

Stomatitis materna has been supposed by some to be caused by a defibrination of the blood from the use of water impregnated with an excess of lime. Dr. McKee remarks, however, that the section of country in which his cases occurred was a high and healthy granite ridge, affording freestone water of an excellent quality. Besides, a good many cases occurred in the rural districts, where the purest air and water exist.

A valuable report is presented by Dr. H. Kelly, on the diseases most prevalent in Iredell County, from May, 1856, to May, 1857.

The report of Dr. J. J. Summerell, on the diseases of Rowan County, contains three very interesting cases; one of fracture of the skull, with injury of the brain. The patient recovered perfectly at the end of three months from the receipt of the injury. A second, of compression, from extravasated blood, of the brain, without fracture of the skull, the result of a fall. The skull was trephined, the extravasated blood removed, and the patient ultimately recovered entirely. The third case we give entire; it is one of delirium tremens following an attack of pneumonia.

In the report of Dr. N. J. Pittman, on the diseases of Edgecomb County, we find some instructive remarks on epidemic dysentery and its treatment, and several instructive surgical cases.

The most extended and elaborate report in these *Transactions* is that of Dr. Otis F. Manson, on malarial pneumonia.

The reporter examines very minutely the general course and progress of pneumonia, as it usually occurs in malarious districts, its physical signs, and the physical changes that occur in the disease; the diagnosis between it and the other forms of pneumonia; its cause and nature, and the treatment best adapted to conduct it to a favourable termination.

The report is one we should like to dwell upon at some length. The author has emitted opinions which are eminently suggestive, to say the least of them, and certainly deserving of a close and candid examination. The correctness of some of his conclusions may admit of question, still as a very masterly history of pneumonia, in the form under which it usually presents itself in those whose systems are labouring under the influence of malarial poison, it forms certainly a valuable contribution to pathology. We can spare space only for the following extracts, from which a tolerably correct idea will be obtained of the general views of Dr. Manson in relation to the true character of what he terms, with, we think, some degree of looseness, malarial pneumonia:—

"The doctrine that this form of pneumonia owes its existence to malarial influence has long been entertained and acted on; but, on the other hand, has been as strenuously contested, both in regard to its correctness and legitimate consequences. The subject is one of great interest and of the highest importance, involving unquestionably the lives of the inhabitants of many sections of our own and other countries. The reader who has had frequent opportunities of observing remittent fever at the bedside, or those who have not, who have known the writer, by a perusal of his imperfect descriptions of that disease, cannot fail to perceive the close resemblance between the phenomena presented by that affection and the disease under consideration. Indeed, the similitude is almost perfect, with the addition of the signs and symptoms of the thoracic lesion. It will be observed, therefore, that the evidences of organic implication are by no means confined to the organs of respiration, that symptoms of gastro-intestinal disorder very frequently preceded the lung symptoms, and that both were very often preceded by febrile excitement for hours and even days. We have seen that the patient evidently does not always perish from the pathological changes wrought in the lungs; death more often occurring from the supervention of fierce delirium, not only independent of the lung affection, but subsequent to its abatement or total disappearance. These facts, then, clearly denote the connection between this disease and periodical fever, as they sufficiently prove that its phenomena are not merely symptomatic of lesion of the lungs.

"By some, an explanation of the existence of this disease has been given by attributing it to the combined agency of malaria and atmospheric vicissitudes. This is doubtless often true, but it does not offer a satisfactory solution of the fact that many are often attacked with it who have not been exposed in any appreciable degree to those changes. Indeed, this affection often attacks a number of persons, consecutively, in the same household, with intervals of time elapsing between the occurrence of the cases to permit all conditions of temperature and humidity to appear properly belonging to the season. 'It is probable,' says Laennec, 'that the epidemic peripneumonia is often owing to the deleterious miasms which have entered the system by means of the cutaneous or pulmonary absorbents, since nothing is more common than to meet with cases of this disease to which we can assign no occasional cause.—How many persons are seized with it in their very chambers, and in spite of the utmost care taken of their health.' It is impossible that any observer of ordinary perception can have failed to be often impressed with this reflection. Exposure to atmospheric alternations does not, therefore, offer a complete and satisfactory explanation of the general occurrence of this affection. I propose, with some diffidence, to offer a few simple thoughts on the subject. It is a fact, which will scarcely be questioned, that during the prevalence of any fever, in seasons of low temperature, pulmonary inflammation is prone to occur as a complication. In scarlatina, rubeola, typhoid fever, and other febrile affections, it is very common. There are some causes, therefore, existing and belonging to the season, which either excite or predispose to lesion of the lung parenchyma. In instituting an inquiry into the operation of these causes, it will be necessary to bear in mind that the system has passed through the fervid heat of summer, the balmy temperature of autumn, by transitions more or less variable, to the colder season of the year. Usually the outward man has been prepared for this change, the entire skin surface having been protected from the effects of reduced temperature by clothing of proper quantity and quality. But how is it with the lungs, the only remaining surface, besides its outlet, which is exposed to the direct contact of air? What has been done to protect that extensive, delicate lining of the air-tubes and air-cells?"

"It is evident that this great annual change of temperature cannot take place without some important physiological revolution in the pulmonary circulation; to understand the nature of which, it is only necessary to remember the usual effects of reduced temperature upon vascular tissues in general. The first impression on the lungs must necessarily be succeeded by contraction of their capillary vessels. Happily, however, our organs are endowed with a certain degree of resistance to depressing causes, under the operation of which the vessels again expand and allow the free transmission of their contents. In short, the equilibrium of health is established. This resisting force which thus averts disease, or, in other words, the nervous force—the only power existing in the system, of which we are cognizant—must, however, be continued and maintained, in order that no embarrassment to the circulation may ensue; consequently throughout the colder seasons a greater amount of nerve power must be expended in the lungs to preserve their physiological condition. Again: the effects of the reduced temperature will be in proportion, 1st, to its intensity and duration; and, 2dly, the capability of resistance residing in the system. If we suppose that the system is contaminated by the depressing influence of malarial fever, or debilitated by its latent influence, it necessarily follows that such a person must be in a favourable condition for the development of pulmonary engorgement, from the fact that he is subjected to a temperature requiring a healthy nervous force to resist, which he does not possess. There is plainly, then, a degree on the thermometric scale, equivalent to disease with him, yet compatible with health in others.

"Let us advance one step further. The degree of cold being present beyond the nervous power to resist, or having endured so long as to have for a time exhausted its resources, let us look again at the capillaries of the lung. If the vessels, contracted by the application of cold, do not possess the normal resistance vigour, as we have assumed as an axiom in the malarial diathesis, the same or greater effect is produced than by a more intense degree of cold, acting

on a healthy subject. Under ordinary circumstances, however, the nervous centres will be stimulated to react—those cases attended with collapse in the commencement being the exceptions—the reaction, according to a law of the organism, being in proportion to the previous degree of depression. 'An increased afflux of blood to the part consequently ensues, and as the action of cold has indirectly produced the same effect as that produced by a direct irritating cause, it will only be necessary to follow the subsequent steps in the process to the state of active congestion, as detailed by Kaltenbrunner's microscopic experiments.' But there is yet another function which has also to undergo an important modification. The skin, which in the warm seasons had been exalted to excessive activity, gradually falls into a state of comparative repose as the bleaker season arrives. This evidently occurs, however perfectly it may have been protected. This subsidence of function must necessarily be followed by a determination of blood to the internal organs, the retrocession being greater and more easily induced in malarial subjects, for reasons before adduced. We have now an explanation of the modes by which cold and malarial influence may act as exciting and predisposing causes of this affection; but this solution does not wholly apply to those cases occurring in previously healthy subjects, whilst sedulously guarded from atmospheric vicissitudes. These cases are evidently due to the evolution of malaria, the affection of the lungs being produced by the predisposing tendencies of the season.

"Objection has been urged on many grounds to this view of the causation of the disease, the principal one of which is that it prevails in seasons inimical to the existence of miasmata. This is plainly an error. Primary attacks of periodical fever not unfrequently occur during the winter and spring months, especially the latter, which is also the season in which this form of pneumonia is usually prevalent. It is admitted that a form of disease similar to that we have described often prevails as an epidemic. Now, whether it be regarded as merely a complication, a compound, or as a hybrid affection, it cannot be disputed that it rages in those seasons of the year, the temperature of which they assume to be destructive to malarial influence. The inconsistency is obvious.

"The occurrence of periodical fever not only in the spring, but occasionally in the winter months, being easily established, there are conditions necessary to be noticed, closely associated with its nature and phenomena, which, besides the tendencies of the season, assist in producing a sanguineous determination to the lungs. In discussing the subject of remittent fever, I briefly endeavoured to prove its nature from the character of the symptoms existing during life, and to show that there was constant tendency in the vital current to retrocede from the extremities to the trunk, and in grave cases from the whole surface to the internal organs. Of the cause of that morbid afflux I then, as now, avoided discussion. My opinion is that it is produced by an irritation of the nervous centres, of a peculiar character. As a consequence of this irritation and congestion, delirium, difficult locomotion, dyspnœa, independent of pulmonary engorgement, with varying conditions of pulse, and abdominal symptoms, were observed, accordingly as different portions of the cerebro-spinal axis may have been involved.

"To sum up our reflections on the pathology and etiology of this form of pneumonia, we have, to explain its production and phenomena—

"1. The predisposing tendencies of low temperatures to produce an afflux to the lungs, by direct contact with their interior surface.

"2. The same cause, by repression of the cutaneous evacuations, determining to the whole internal organs.

"3. The operation of malarial influence, which not only diminishes the vital resistance to cold, but possesses an evident inherent tendency to produce internal engorgement."

"I have repeatedly stated my impressions in regard to the condition of the lung parenchyma in this affection, as partaking rather of the nature of active congestion than well-developed inflammation. This is proved, in my opinion, by the following circumstances: 1. The sudden occurrence of solidification, as evinced by perfect flatness on percussion, and absence of the respiratory murmur. This sometimes takes place, in grave cases, in a few hours after its onset,

involving more than half of the entire lungs, accompanied by a cold skin, flagging pulse, and colliquative diarrhœa, a condition of system totally incompatible with the existence of the inflammatory process. 2. The disappearance of the physical signs is too rapid in many cases to suppose that inflammation, terminating in plastic exudation, had produced this impermeable condition of the lungs. It is true, as we have stated, that there are some evidences of inflammation mingled with the symptoms; and as active congestion is a state closely allied to inflammation, the latter may, and doubtless does, occur; but it is evident that inflammation cannot be the general condition, as it is too expeditiously removed, in many cases of several days' duration, and by inadequate means, to render that view admissible."

The annual address of Dr. S. S. Satchwell is a most able one. His picture of medical science is admirably and truthfully drawn, while his exposition of the causes which have in some measure impeded its progress, and reduced in no slight degree the standing of the medical profession in our midst, are clear, faithful, and impressive. We regret that want of space will not allow us to present some extracts from this excellent address.

4. The present volume of the *Transactions* of the Pennsylvania State Medical Society is, in several respects, less interesting than some of the preceding ones.

The annual address, delivered before the Society, by its President, Dr. R. La Roche, at its session of May, 1857, is among the most able and interesting of the large number of productions of the same class, we have had an opportunity, for several years past, of perusing. It is replete with important truths that cannot be too often brought to the notice of the members of our profession, or too strongly urged upon their attention. Could the public in general be brought to a full recognition of the leading principles adduced and ably enforced by Dr. La Roche, it would be equally to their advantage as to that of the well instructed, skilful, and honourable members of the medical corps; for scarcely less interested is each individual of the State in the elevation of the professional and moral standing of the medical profession, and the accurate definement and proper protection of its legitimate rights, than are its own members; inasmuch as all must necessarily derive from the enlargement of medical science and skill, through the labours of an educated, experienced, and faithful body of physicians, recognized and cherished by enlightened public opinion, increased security from the causes of disease, on the one hand, and on the other, a greater certainty of prompt and entire recovery from its attacks.

Dr. La Roche commences his address by a striking and faithful delineation of the importance to the profession of a complete and efficient organization of its members in each locality, county, and State. He points to the well-recognized fact that from associated action among those engaged in the same pursuits, far greater advantages are to be obtained, and more important results with certainty secured, than can possibly be anticipated from individual efforts, however energetic and multiplied, and whatever may be the professional and social standing of those by whom such efforts are made. The divided action of the largest number of intelligent and well-instructed physicians, spread over the broad surface of the State, can never accomplish the same amount of permanent good to the profession generally, as is certainly to be obtained when they act in concert and for one common object. The isolated actors lack "the vivifying influence of emulation, the stimulus to exertion springing from the co-operation or collective action of many minds equally interested in the attainment of a desired end; they miss the opportunities of improvement flowing naturally from the interchange of thought, and personal intercourse with such of their brethren as enjoy ampler means of acquiring a full knowledge on all, or some special branches of medical science, not less than on the condition and wants of the profession, or who, from their fields of observation being more or less remote from each other, are enabled to see facts under the diversified aspects they are too apt to present." In a word, they eventually fail in securing that amount of good at which they aim, by endeavouring to work it out

alone, and for themselves, irrespective of the common interests of all who are engaged in the same pursuits.

From medical organization "other benefits than those having reference to the general bearing and conduct of medical men living within the sphere of their jurisdiction are looked for. As already pointed out, such associations tend in various ways to elevate the character, honour, and dignity of the medical profession. It is not enough, in order to attain this desired object, that laws and regulations for the proper guidance of their respective members should be enacted and enforced through means of the connected action of properly constituted bodies. It is not enough that, by such bodies, all irregularities occurring in the professional and social bearing and conduct of such members should be corrected, and the repetition of them prevented, and that the requisite means to remedy the evil accruing therefrom should be indicated. A certain degree of influence, both direct and indirect, must be brought to bear on those members individually, with a view to stimulate their zeal and energies to the vigorous pursuit of inquiries of an intellectual character. They must be roused from the state of mental inertia in which, generally speaking, men are apt to fall, when deprived of external stimulus. They must be saved from that condition of stagnation which, 'in the moral, as in the physical world, generates corruption.' Again, they must be made sensible of the importance of collecting and arranging systematically, useful facts of a pathological, etiological, and therapeutical kind, and of communicating these, and the deductions drawn from them, through some channel or other, to the medical community at large."

Dr. La Roche urges with great force the necessity of a complete classical and scientific and professional education in all who would enter upon the high and responsible duties of a physician; and of continued, uninterrupted study subsequent to their entrance, to render them familiar with the vast store of facts and principles contributed by those who, in times past, have investigated with success the several branches of medical science, or which are now being contributed as important additions to the common stock of professional knowledge.

"What are called plain and simple practitioners—men who contemptuously look down on the industrious student, and place him in the same category with sheer bibliomaniacs; who ignore book-learning, and stigmatize as useless theorists, all physicians disposed to attach importance to an inquiry into the causation, nature, and relationship of pathological facts; who affect to rely exclusively upon bedside experience, or what they characterize as simple observation; who care little for the results of the experience of their predecessors or contemporaries; smiling at the idea of the necessity of comparing the phenomena they observe, and the therapeutic results they obtain with those that fall under the cognizance of their fellow practitioners, and greatly doubting the utility of pathological anatomy and other kindred branches of medical knowledge; such physicians, I say, are not, as a rule, the instruments employed by Providence to promote the progress of medical science, elevate the character and secure the dignity of the profession.

"They may succeed, and have, doubtless, often succeeded, here and elsewhere, in becoming safe practitioners. They may learn to manage in a satisfactory manner certain classes of stereotype cases of morbid derangements, which they daily encounter. They may in that way succeed, ultimately, in becoming useful to those who honour them with their confidence, and also, not a little to their private interest—eliciting the applause, and commanding even the admiration of a large portion of the community, who, in total ignorance of the true requirements of a physician, sing the praise of simple and unsophisticated experience, and deny the necessity of theory, which they stupidly confound with hypothesis, while they regard in the light of useless and dangerous dreamers, all who aim at something beyond the standard they have fixed upon as the essential qualifications of a medical practitioner. They may do all that, but I hesitate not to say, that he who looks to them for aid in furthering the progress of medical science in any of its multifarious departments, will be very much deceived. Even practical medicine, on their skill in which they pride

themselves, receives but slender benefits at their hands. Physicians of that class are at a loss whenever they encounter cases such as they had not seen before. Even the less usual modifications of ordinary complaints, are but too often to them a source of embarrassment—not unfrequently a rock on which they founder. Or, if, through means of an unusual degree of native sagacity and medical tact, or a constant habit of the sick room, they succeed, at last, in surmounting some of these difficulties, certain it is, the number of such fortunate physicians is exceedingly limited, and whatsoever be their success, they teach nothing to their own, and particularly to future generations. They never prove benefactors to science. The latter, in their hands, never advances beyond the point at which they found it. Indeed, I am not sure that, if left to their fostering care alone, it would not retrograde. Medicine, with the greater number of such routine and empirical physicians, is not a science. It is not even an art. It is a trade. Content with finding a remedy for the disease for which they are called upon to prescribe, or for some particular phenomenon; employing this remedy because they think it has been useful in some instances of like kind, they ignore principles; or if they raise the pretension of being guided by pathological views, it is ten to one that such views are not founded on sound principles, and are calculated to excite a smile from all well-informed physicians."

"Nothing, it has always appeared to me, is more devoid of force—I had almost said, more senseless—than the opposition that is so often made to what is sneeringly denominated book-learning, and the reason assigned to justify that opposition—*i. e.*, that a physician who spends much time in reading; who is learned in books, and especially who cultivates general literature, cannot, on that account, become an experienced and successful practitioner. Doubtless, as said before, an exclusive devotion to information derived through such means, and a neglect of that furnished by an assiduous attendance at the bedside of the sick, would lead a physician to results of a doubtful character. But, because such a mode of proceeding would never enable him to acquire the clinical experience requisite to render him useful in a practical point of view, it does not follow, as is but too often asserted by some physicians, and is still more generally echoed by the public, that a reading physician cannot become skilful and successful in the practice of his art. So far from this, when restricted within reasonable bounds, and combined, as it should, and may easily be, with clinical knowledge, that much abused book learning generally proves of the most decided advantage to the practical physician, and places him in a much higher position in respect to his usefulness as an attendant at the bedside. The history of our profession furnishes us with numerous cases in illustration of the truth of this statement, for we therein find, that among the greatest practitioners and clinical teachers whose names adorn its pages, a large number were well and even deeply versed in the literature of the science—many in general literature; while not a few of those who from want of time or inclination, had neglected to store their minds with information derived in the way mentioned, would evidently have avoided many errors, and risen higher than they have done in the estimation of their contemporaries and successors, had they followed a different course. A well read physician has more resources at his command when placed at the bedside of the sick. Knowing what has been observed and done by others, as regards the treatment of diseases which fall under his observation, as well in their pure as in their complicated states; aware also of the results of the researches made in various places, and under diversified circumstances, by individuals competent to the task, relative to the nature and seat of those diseases—the symptoms by which they can be detected, the signs by which they may be diagnosed, the treatment and organic changes they occasion, or which reveal their true nature—he is better prepared to combat them with success, whatever be the modifications under which they present themselves, and, instead of groping as it were in the dark, resort to the means found useful in the management of cases similar to those before him, but which he may not have already seen in their present aspect. He is more certain to arrive at a correct diagnosis and prognosis even in cases, or modifications of cases, of unusual occurrence. He is less apt to find himself at a loss in moments of emer-

gency, and more adequate to take an enlarged and correct view of the causation and nature of the diseases under his charge, and to deduce from his observations principles calculated to guide him successfully in the selection of his medical means. To this let me add, that a physician of the class under consideration, is less in danger of merging into the mere empiric, or of lapsing into quackery than the indiscriminate condemner of book learning. The latter contributes little or nothing to the progress of even practical medicine. They live, they die, they are forgotten. At the time of their disappearance from the stage of life, the amount of practical resources at their command is scarcely greater than it was when it commenced, except as is afforded by a certain degree of familiarity with the salient phenomena and the progression of diseases commonly encountered by them during their rounds, and with the effect in the treatment of these, of common remedies. They leave nothing likely to benefit others. They are besides seldom fastidious in regard to professional ethics, and are not in general looked up to as beacons for the guidance of their brethren in their endeavours to elevate the medical profession in the estimation of the public at large."

Dr. La Roche enforces with great truth the necessity of a certain amount of enthusiasm, as a necessary element of success in the physician who would perform well his duties to those placed under his professional care, and contribute effectually towards the advancement of medicine as a science and an art. In the conclusion of his remarks on this subject, he says:—

"I have said enough to indicate my belief that a physician cannot hope, and is not destined to attain, the object in view, still less to acquire an eminent position among his brethren, and contribute to the advancement of any one of the several branches of the science he cultivates, unless he be endowed with the love or enthusiasm of science, and enters into the pursuit of the knowledge necessary to the proper performance of his professional duties with more than a common alacrity. Open the records of the profession, and task your memories respecting the physicians you have heard or read of, or whom you are now personally acquainted with, and you will agree with me in the opinion that all who have contributed to the results mentioned; all who are, or deserve to be numbered among the ornaments of our profession, who have obtained a high position among their compeers on the score of their attainments as physiologists, pathologists, anatomists, chemists, botanists, &c., were, or are endowed with a greater or less share of the enthusiasm in question. Even those who have pursued with success the department of clinical medicine, and who usually are regarded by the public at large, and a certain class of practitioners, as the beau-ideal of physicians, and as belonging to the only useful class of medical men, on the score of being, as it is thought, more practical than theoretical in their tendencies—even they have reached their eminence in the ranks of the profession through the influential agency of enthusiasm. For medical observation is not simply the result of a passive operation of the organs of sense. It requires, in order to lead to useful issues, a force of mind, a quickness of perception, a degree of penetration, and hence efforts of the intellectual functions—a constant and rapid appeal to the faculties of causation and comparison, which cannot be exercised to the required point unless under the stimulus of enthusiastic excitement. Had he not been under the control of the agitating and impulsive feeling in question, Hippocrates would not have been remembered in after times. Nor would Galen, Celsus, Avicenna, nor Boerhaave, Harvey, Stoll, Selle, Cullen, J. Hunter, Broussais, Louis (I mean he of Surgery), nor Bichât, Rush, Tommasini, and a thousand others I could mention. None of these, I repeat, could have attained the eminence they occupy, and stamped their names in imperishable letters on the pages of our professional history, had they not felt the vivifying effects of the sacred fire in question."

The comments of Dr. La Roche on the subject of medical education as it is conducted in this country are sound and pertinent.

The following are a portion of the very just though severe remarks in the address before us, on the subject of American medical literature:—

"It is said that the character of a nation may be ascertained from the nature of its literature. With equal propriety we may affirm that the position, honour,

and dignity of a profession may be estimated by the same standard. If submitted to this criterion, we are compelled to admit that the reputation of the medical profession in this country will not stand the ordeal; for we discern nothing in our literature, viewed in its totality, calculated to shed lustre on our calling—much, on the contrary, to affect injuriously its honour, and position, and lessen its dignity. I have said, in its totality, for in several of its departments that literature, though in its infancy, when compared with that of Europe, has attained a degree of eminence of which every American physician may justly be proud. My remarks apply more especially to the periodic medical press, in which, if we except some journals and reviews, that would do honour to the literature of any country, and are fit representatives and supporters of the character and respectability of the profession, are exhibited an absence of solid knowledge, a jejuneness, a lack of useful materials, a prolixity about things perfectly unimportant—which everybody knows, and often for which nobody cares—and very generally a deficiency of refinement, good breeding, and literary culture, little calculated to aid in raising us in the estimation of the scientific world, and in promoting the attainment of the desirable object to which allusion has so often been made. Every place must have its journal—sometimes more than one; every physician thinks himself qualified to edit a journal. It not unfrequently happens that he has never seen or treasured up in his recollection anything worth communicating, and that he has no one around him competent to aid him in the undertaking. As frequently it occurs that he is located in a position where facts of a novel, important, or interesting character are never encountered; or when so, are at best not sufficiently numerous to furnish food for more than a short annual communication. As often his pursuits have not been, and the appliances within his reach are not such as to enable him to carry on pathological, therapeutical, or other investigations in the way best calculated to render them truly useful and acceptable to medical inquirers. It may happen, also, that he, no more than his colleagues, if he has any, is an adept in the art of composition, and that he is deficient as regards the simplest elements of a correct and polished style. It is not unusual, besides, to find that he, for the most part, limits his reading to a few text-books and sundry journals of a merit equal to that of his own bantling, and that in point of tone, taste, and discretion, he can lay but slender claim to the approbation of cultivated minds. But no matter—his place must have a journal, and he must be the editor. He is full of an idea of his importance, and blind to his many disqualifications to perform with proper efficiency the duties he has undertaken. So, his production is launched forth into the literary world to the no small injury of the entire profession, of whose intellectual and scientific position, no less than of its honour and dignity, it is but too apt to be viewed by the medical critics and readers of Europe, as by a large portion of the public here, as a fitting exponent.

"We could, of course, scarcely expect to find, in such journals, judicious, well written monographs on important medical topics, or a detail of interesting cases establishing their true pathological character or therapeutical indications, or other communications, exhibiting a correct knowledge of the subject matter. Equally futile would it be to search therein for fair, elaborate, and learned reviews of recent publications, temperate and courteous in tone, however severe in criticism. But while such things cannot well be looked for in the kind of journals here referred to, we might expect that the better sort of periodical publications, and especially those of a high order, published in cities of some literary pretension, would escape the charge of being, occasionally, at least, receptacles of essays, and especially of reviews, of an indifferent and otherwise objectionable kind. This, however, I regret to say, is not the case, for instances might easily be pointed out in which the pages of those journals have been disfigured with trite, unmeaning, childish, so-called original articles, and with bibliographical notices and reviews, in which American and other works of merit and importance are mentioned in the most flippant and trifling manner, or dismissed with a few words or lines suited rather for a publisher's advertising sheet than the pages of a scientific journal; or, what is worse, in which the authors noticed are treated most unfairly and uncourteously—repre-

sented sometimes as saying the reverse of what they have really said—turned into ridicule on points of doctrine or practice on which the reviewer happens, ten to one, wrongfully, to differ with them—taxed roundly and unceremoniously with want of experience or knowledge of the subject under consideration, and not unfrequently charged with falsehood and insolence for uttering sentiments unpalatable to a certain clique or favourite writer, or a popular teacher."

We regret that we cannot, without encroaching upon the space destined for other articles that claim admittance in the present number of the *Journal*, indulge in any extended quotations from that portion of Dr. La Roche's address in which, with merited severity, he scourges the rampant quackery with which, under multifarious forms, every portion of our country is overrun and disgraced. We can venture only a single sentence, the bitter satire of which is but too well merited:—

"Boileau tells us of an individual who '*de mauvais médecin devint pire architect.*' In our beloved country, we often see bad shoemakers, or the like, becoming worse doctors. In the former case, the charge was nothing but an unfounded sarcasm; for the bad physician who, according to the French satirist, became a worse architect, was no less a personage than Perrault, and he, whatever may have been his medical deficiencies, will certainly be admitted to have exhibited no slender claim to respect in his new avocation, seeing that he embellished Paris with the magnificent *façade* of the Louvre. But had the charge of degeneration been well founded, there would have been this difference between his case and what happens among us: that the bad architect would have obtained no employment in his new line of business—certainly would not have worked on the Louvre; whereas, our bad shoemakers, who become worse doctors, command the confidence of the public, and generally reap a rich harvest."

The first of the reports is one on vaccination and vaccine virus, by Dr. Henry Carpenter, of Lancaster. There are many questions connected with the subject of vaccination of deep interest to the physician, and in the correct settlement of which the safety of the community at large, from the introduction of smallpox in its midst, and its more or less extended spread, is intimately involved. These questions are rather indicated than discussed in the report before us.

To render vaccination a certain prophylactic to the variolous contagion, it is necessary not merely that each individual in the community should, at as early a period as practicable after birth, be subjected to its influence, but, also, that the utmost care be taken, in each case, to place the system fully, effectually, and permanently, under its protective influence. Now, to effect this, it is essential that none but genuine, efficient matter should ever be employed in vaccinating; that the insertion of the matter be carefully and properly performed; and that the progress of the arm from the period when the matter is introduced into it to the full development of a genuine vesicle, its maturation and desiccation, be attentively watched by one competent to decide as to the genuineness and completeness of the infection. Many circumstances may occur, even after a true vaccine vesicle has been developed, to prevent that complete infection of the system being secured, from which alone entire protection can be anticipated. The evil resulting from an imperfect or spurious vaccination, does not stop with the endangering of the life of the patient in whose case it happens, but it endangers, through the susceptibility of such patient to subsequent variolous infection, the introduction of smallpox into a community, and the lives of all such of its members as are still unprotected. It furthermore impairs the confidence of the community in the value of vaccination, which of itself is an evil of no slight magnitude.

Now, to insure the success of vaccination as a complete and permanent protective against smallpox, it is essential that in every case pure, efficient, and genuine vaccine virus be employed. That there is in common use vaccine matter which has become to a certain extent deteriorated or effete, and which, in consequence, affords, when introduced into the system, only a partial or temporary protection, is undeniable. The fact is admitted by physicians of experience and close observation, in every section of our country, and the consequent necessity for the adoption of the proper means for securing a renewed

supply, at proper intervals, of genuine, effective matter has recently been largely discussed. To secure such supply, we know of no other certain plan than a resort to the original source of the vaccine virus—the cow. We are convinced, from the successful efforts we have ourselves made, that by the agency of such of our county medical societies, as are located in the immediate neighbourhood of extensive grazing and dairy districts, pure vaccine matter direct from the cow, could be collected with but little trouble or expense, in quantities fully adequate to the supply of all the physicians of the State, and that a plan could be devised for the proper and regular distribution of the matter thus obtained, at once simple and efficient. Upon the societies referred to we would urge a consideration of this subject; we trust that they may be induced to take it in hand without delay. By carrying it into effect they would secure the thanks of the profession throughout the State, while for whatever trouble and expense they may incur in its effectual prosecution they may be adequately remunerated by a trifling charge on the different portions of matter distributed by them.

We do not pretend to assert that vaccine matter degenerates in activity and efficiency in proportion to the number of human bodies through which it is made to pass. We have repeatedly seen the most prompt and genuine infection produced by matter that had been propagated through the human subject for a long series of years. But this we do assert, that the longer vaccine matter is thus propagated the greater is the chance, from a great variety of causes, some unavoidable, and others the result of carelessness and ignorance—of its becoming deteriorated and inefficient, and that the only certain means of keeping up a supply of genuine, effective vaccine matter, is by renewing it occasionally at its original source.

It may not be improper to remark, that even universal and apparently efficient vaccination, performed with unquestionably pure, genuine, and active matter, will fail in securing the community against the occurrence of smallpox, in a modified form at least, unless each individual vaccinated be subjected to the test of revaccination. It is a fact, now well attested by the most extensive and repeated series of observations, that there exists a wide difference in the constitutional susceptibility, not merely to the infection of the vaccine virus, but also to that peculiar constitutional influence or change by which its influence is rendered permanent. In many subjects the effects of the vaccine impression would appear to be entirely local, while in others, notwithstanding the impression evidently extends to the system at large, this general impression is either not sufficiently intense to give complete protection, or it is, after a shorter or longer period, entirely dissipated, and thus leaves the system fully susceptible to the contagion of smallpox, when exposed to its influence. The importance, or rather the absolute necessity of revaccination as a test of the completeness and permanency of the first infection is therefore apparent. How soon after the first vaccination, the second operation should be performed, or how often it is necessary to resort to revaccination, are questions we do not propose to discuss here. Until a system of registration in respect to vaccinations, accessible to the medical profession at large, shall be agreed upon, and carried into full effect, by which we shall be enabled to judge of the validity of the operation in each case, our course would be to revaccinate, without distinction, all who are exposed to variolous contagion, either from the occurrence of smallpox in their immediate neighbourhood, or as an epidemic in the community amid which they reside.

The *Transactions* of the Pennsylvania State Society comprise reports from nine counties. These reports vary greatly in extent and interest. There is none of them, we regret to say, that can claim any very high character for the ability with which it is drawn up or the value of the information it communicates. So far as we are able to form from these reports any judgment of the general health of the State during the year 1856, this would appear, upon the whole, to have been particularly favourable. There was a general absence of severe, malignant, or wide-spread epidemics, and even the usual endemic diseases of the several sections of the State included in the reports presented were of much less frequent occurrence and far milder in character than ordinary.

The report from Chester County is preceded by a very valuable account of the geology, the medical topography, and the meteorology of the county; the one from Washington County presents a similar account, of great interest, but less full and precise.

To several of these reports are appended biographical notices of members deceased during the year, a practice that we should like to see more uniformly followed.

In the report from Beaver County we find the following remarks in reference to the occurrence of an epidemic of dysentery:—

"In 1834, a year remarkable for a very extraordinary severe frost, which fell so late in the season that all the fruit of every kind was killed, not even any of the wild fruits escaping—in many places, on the low grounds, the forest timber was much injured by the frost—dysentery prevailed in the months of July, August, and September, in a manner unknown either before or since, both as regards its malignancy and the extent of country over which it spread. It was much worse, however, on the south side of the Ohio River. A creek of considerable size (Raccoon) passes through the entire county on the south side, emptying into the river about three miles below Beaver. Along this stream, in the bottom and on the immediate hill-sides, there was not a family that escaped this disease. It frequently ran through all the family.

"In relation to this epidemic, the most interesting inquiry is its cause. What was the cause? The section of the country where it prevailed in its most malignant form remains unchanged, wearing the same general appearance it did twenty-three years ago. There have been no improvements that could possibly affect the health, no ground of any consequence cleared. Notwithstanding that nearly twenty-three years have rolled round, with all the diversity of seasons occurring in that long period, yet that district has been entirely exempt from dysentery; scarcely in all this time has an isolated case occurred. The immoderate use of unripe fruit is very frequently assigned as the cause of this disease. As there was no fruit of any kind in 1834, it could not, of course, be attributed to it. Is it not probable that this total want of fruit was one cause of the disease, its place being supplied by the crude, immature products of the garden and field? It is not at all probable that fruit, used even in excess, is the cause of dysentery." "The universal fondness that all children have for fruit would seem to indicate that it is necessary for their health. Our natural healthy *instincts* are never wrong."

In the report from Perry County, Dr. Lefever states that he was informed by a gentleman of his acquaintance, but not of the medical profession, "that he had been in different parts of our country where dysentery prevailed as an epidemic, and during some seasons many of the cases terminated fatally; and in every instance he had observed that fruits were very scarce in the particular section of country in which the dysentery prevailed. From this fact he was led to believe that, instead of being the *cause* of dysenteric affections, an abundance of fruit was a *preventive* of such diseases. From my own observations, I am not able to express an opinion as to the probable truth of the position mentioned; but I am inclined to believe that even the free use of good, ripe fruits will rarely prove injurious, and might oftentimes be permitted to those who are unwell, where physicians are in the habit of forbidding it, because such has been the custom. Where patients desire it, I have frequently allowed ripe fruits in moderation in cases of disease, and very seldom, indeed, have I had any reason whatever to believe that the slightest injury resulted from the license given; but, on the other hand, I believe I have frequently seen good results follow."

Returning to the report from Beaver County, we are informed that "epidemic cerebro-spinal meningitis prevailed there in the spring of 1848, in a very malignant form. The attacks were mostly sudden. The patient would be seized with violent pain in the head, most severe in the occiput, but frequently being felt in the temples and over one or both eyes; the pupils of the eyes were mostly contracted, but sometimes dilated; great intolerance of light; the brows knit; throbbing of the carotids; severe pain in the back of the neck and along the spine; in many cases violent opisthotonos; during the paroxysm the body

would be raised entirely from the bed, the head and heels alone touching; delirium and coma in the advanced stages; obstinate constipation of the bowels, it being with great difficulty that evacuations could be produced; the functions of the kidneys were partially or totally suspended. Many cases proved fatal very speedily, whilst others would linger until the patient would become extremely emaciated; in the latter cases there would frequently be a total loss of sight and most of the other senses. We are not aware that any *post-mortem* examinations were made, the people in the country being greatly opposed to anything of the kind."

In the report from Blair County it is remarked that "intermittent fever and ague were hardly known along the Juniata this year. During the preceding year (1855) seven-tenths of the residents of the river valley had intermitting fever or ague, or both; while, at the same time, in Franktown Township, it extended up the small tributaries of that river for several miles, into localities where it had never before penetrated, spreading dismay among those unacquainted with it. That whole summer the channels of the river and its tributaries were kept well filled by frequent rains. On the contrary, the summer of 1856 was very dry, the streams very much dried out; so that a large surface of their channels, with whatever of vegetable or animal matter they contained, was exposed to decomposition. Why in 1855, when all the conditions were those considered unfavourable for the extrication of malaria, intermittents should be so extremely rife, while in 1856, when the conditions seemed altogether favourable to the production of malaria, those diseases should scarcely exist, is a question somewhat knotty of solution."

Dr. George W. Brown, in the section of the report from Schuylkill County devoted to the consideration of the diseases of women and children, tells us that when he left college he was fully satisfied, from having listened to Professor Meigs's very emphatic assertions in regard to the treatment of puerperal fever, that he ought to bleed every lying-in patient that was seized with a shivering fit, followed by pain in the belly, and fever, without regard to her constitution, and he did so till he nearly lost several of his patients by syncope, and produced long and tedious convalescence in others, when he began to doubt its indiscriminate applicability; since then he has come gradually to use his own judgment, and has bled only such cases as were strong and vigorous, and when the symptoms were clearly of an inflammatory character, and trusted the others to the internal use of calomel, opium, pulvis antimonialis, and oil of turpentine, and he thinks he has abundant reason to be satisfied. After an experience of thirteen years, and an attendance upon nearly two thousand cases of midwifery, in a pure, invigorating atmosphere, among a class of patients who, as a general rule, are well fed, and from a pretty ample attendance upon cases of childbed fever, he is satisfied that it, like all other diseases, is of two kinds, viz., inflammatory and typhoid, and that these require essentially a different treatment. He is also perfectly satisfied that the disease is at times contagious, and that the physician is oftentimes the unsuspecting medium by which it is conveyed from patient to patient. He is fully satisfied that he has several times introduced it into his practice by attendance upon bad cases of erysipelas, and thinks he has often got rid of it by carrying in his pocket a bottle of Labarraque's solution of the chloride of soda, and washing his hands with it each time before and after an examination of his patients.

In the report from Montgomery County, Dr. Corson complains of the paucity of details presented in the several county reports in reference to the treatment of the diseases ordinarily met with, whether in its results it was found successful or otherwise. To a certain extent we admit that the complaint is well founded.

With a view to obtain information on this point, a series of questions were propounded to the different members of the Montgomery County Society, which, with the answers received, are given in the report before us. Now, in reference to information obtained by replies to such general questions as—Do you bleed in fevers and inflammations? What is your treatment in this or that disease? Do you prescribe mercurials in any disease? Do you employ alcoholic stimulants in typhoid fever or other affections? and the like, one or two important

cautions must be observed. Before any safe conclusions can be derived from the answers given, it is essential that these should not be simply in the affirmative or negative, even if they at the same time present the result of treatment, whether favourable or unfavourable; the answers must, in order to be of any value, include also an account of the particular character of the disease in each case prescribed for; the age, sex, constitution, occupation, and personal habits of the patient; the character of the location in which he resides, and the construction, position, and domestic condition of the house he occupies; and, even further still, the morbific constitution of the season during which each case occurred.

The prevailing error in reference to special therapeutics is the supposition that there is an invariable treatment, a particular class and succession of remedies, adapted to each disease; that the name of a disease being determined, the nature of the exact means adapted for its cure follows as a matter of course. The fact that a certain physician, in a certain place, cured nine-tenths of the patients affected with any given malady with the use or without the use of depletion, mercury, or stimulants, is no safe basis upon which to determine the propriety and success of a similar course of treatment in the same disease in a subsequent year or at a different locality, or in a different class of patients.

The precise therapeutic effects of all our remedial agents being well established by cautious and repeated observations, they are to be applied to the control of disease according to the special indications presented by each individual case. It is in this application—it is in the judicious use of the remedies at his command best adapted to the control of each case of disease that presents itself, in connection with his facility in arriving at a prompt and accurate diagnosis, that the true skill of the practitioner is exhibited. The physician who tells us that he treats this or that disease by such and such remedies is at best a mere routinist, and his experience is valueless as a guide to direct others to the successful treatment of the same malady as it presents itself in a different class of patients, at a different season, or in a different section of country.

D. F. C.

Art. XVIII.—*Catalogue Raisonné of the Medical Library of the Pennsylvania Hospital.* By Emil Fischer, M. D. Printed by order of the Board of Managers. 8vo. pp. 750. Philadelphia, 1857.

In the year 1762, the celebrated Dr. John Fothergill presented to the Pennsylvania Hospital *Lewis's Experimental History of the Materia Medica*, "for the benefit," as the record says, "of the young students in physic who may attend under the directions of the physicians." Upon this hint, the managers acted, for we find them in the following year originating the library, the catalogue of which is now before us.

Even at this early period, the hospital, under the teaching of such men as the two Bonds (Thomas and Phineas), Thomas Cadwalader, and Cadwalader Evans, had become famous for its clinical instruction, and the wards of the house were so largely attended that the managers thought it necessary to demand an admission fee.

According to the then custom of British hospitals, all funds thus accruing belonged exclusively to the attending physicians and surgeons; these gentlemen, however, with a noble generosity, yielded all claim to the money, and proposed to apply it "to the foundation of a medical library for the advantage of the pupils of the Institution." Truly, we should remember these men of old with grateful remembrance. They were men not of the common stamp, endowed with all the medical learning of their time, and with a Christian philanthropy never surpassed, they stand out in bold relief as models for imitation. To them and to their immediate successors, Philadelphia owes the high position to which she has attained in the medical history of our country. They made her, as has been happily said, "the very atmosphere of medicine."

For years after their departure, our medical institutions felt the impress of their character, and it might be readily shown that, in proportion as *these* have departed from *their* high standard, just in that proportion have they suffered in reputation and standing. What has been so eloquently said of others, may with greater force be said of them. "They have passed away and we have entered into their labours. But, though dead, they yet speak to us in that which they have left us. They speak to us in the noble reputation thay have acquired for our institutions, and which we are bound to maintain. They speak to us in the ardent zeal of their pursuit after truth, which they call upon us to imitate, and again they speak to us in the individual celebrity which they acquired, and encourage us to similar attainments. Let us then bow low at the mention of their names, and bless God for giving us such an ancestry."

The library thus founded increased by liberal donations and judicious purchases, made chiefly by Dr. Lettsom, of London, with great rapidity.

The first catalogue was printed in 1790, when the number of books was 528 volumes. This was followed, in 1794, by an additional part. A catalogue was again issued in 1806, to which was added a supplement in 1818. In 1827, in consequence of the very large accession of books to the library, another edition of the catalogue was considered necessary. This was compiled by W. G. Malin, then librarian (now the very able and efficient steward of the Institution), and embraced all the previous catalogues. A supplement followed this in 1837, the library then numbering 7,300 volumes. Since that time, the average number of books yearly added to the collection has been 160, so that it contains at present 10,500 volumes.

Thus it is seen how, by judicious management, has been gathered around a single volume—the gift of Fothergill, the largest and best collection of medical books in the United States—a library worthy of the medical metropolis of the Union, and no unfit monument to the memory of the great and good men, its founders.

After this short and imperfect account of the origin and present state of the library, we shall allow Dr. Fischer to introduce his volume to our readers in his own words. "The present catalogue is an entirely new work, and has been compiled independently of its predecessors. For the old plan of disposing the works in the alphabetical order of the names of their authors a classified arrangement according to subjects has been substituted. The advantages of a catalogue raisonné are so evident that it is hardly necessary to particularize them. Not only the student who is anxious to familiarize himself with the depth and breadth of medical literature, but also the medical writer in his laborious research for authorities which he might consult on the subject of his investigation, will derive material aid from a work of this kind."

The arrangement adopted is that of the catalogue of the Royal Medical Society of Edinburgh, which was published in 1837, the year in which the Society reached its centenary period.

In selecting this as his model, Dr. Fischer did well, for undoubtedly it is the best "digested" or "methodized" catalogue ever published. It is not, however, a catalogue raisonné, and we regret that the Dr. has retained its very objectionable title. A catalogue raisonné, properly so called, is one which gives under each book, worthy of such a note, a bibliographical, critical, and literary notice of the volume. Without doubt, it is the best plan for the collection of an amateur, but is altogether too bulky for a large public library.

In support of what we say, we may refer to Ch. Nodier's *Noveaux Mélanges*, Paris, 1844. This is a *catalogue raisonné* of some 1200 volumes, yet it forms a goodly sized 8vo. vol. of 500 pages. At this rate, our volume would have reached about 4375 pages.

Should any of our readers feel disposed to look into the different systems which have been devised for catalogue-making, we cannot do better than refer them to Gabriel Peignot's *Dictionnaire de Bibliologie*, 3 vols., Paris, 1802–4, where they will find the subject discussed with all the *gusto* of a true bibliomane, and where they may learn *this*, if nothing else, that it takes something more than a mere scrivener to draw up a good catalogue.

We shall now say a few words on the volume before us. Following his model, Dr. Fischer divides his catalogue into four parts; the first relating to works on medicine; the second to those on science, including physics and chemistry, natural history and psychology; the third to those on general literature; and the fourth to miscellaneous works not referable to any of the preceding heads, and the journals, reviews, and other periodical publications; then comes a very full and complete index of the names of authors.

The department in which we are most concerned—that of medicine—is subdivided into chapters in the following manner: Anatomy, Human and Comparative; Physiology; Materia Medica and Pharmacy, including Hygiene and Therapeutics; General Pathology and Practice of Physic; Surgery; Midwifery and Diseases of Women and Children; Medical Jurisprudence and Medical Police; and Medical Literature.

Under each of these heads, the respective works are carefully and ably arranged, exhibiting a very considerable acquaintance with medical literature. The arrangement is so complete and full that the student can readily put his hand upon the principal works which he may wish to consult, in order to enable him to follow up any branch of inquiry. In this respect, the catalogue is invaluable, not only to the student, but to every member of the profession who can get access to the library.

We might further add that the classification and subclassification of the different chapters coincide, as far as it could be done in a work of this kind, with those adopted in systematic treatises on the different branches of medical and general science. Only in those instances where no scientific ground for classification could be found, the alphabetical arrangement has been had recourse to.

In going over the catalogue, which we have done with some care, we have been struck with the small number of German and Italian works on medicine; of French and English medical literature, on the contrary, there is an excellent representation; and we have to express our regret at meeting with so few of our own medical journals of the present day. Surely this is a mistake that should be remedied. Journal-literature should ever form a prominent department in all medical libraries; especially should this be the case in our country, in consequence of its vast extent and great diversity of climate. Of the older journals, several of the sets are incomplete; these should be completed without delay, as many of them are becoming so scarce that, in a short time, it will be impossible to do so.

We have thus hastily brought the volume before us to the notice of our readers. Any recommendation of it would be superfluous. We believe that its existence can no sooner be known than its great value will be recognized.

To the Board of Managers of the Hospital we are greatly indebted for this work, and we congratulate them and the profession on the creditable and useful manner in which it has been "gotten up." They were most fortunate in obtaining the services of one so well calculated as Dr. Fischer to undertake a work so laborious and difficult. None but those who have been engaged in such an undertaking can in any way appreciate the enormous labour which it entails. "Of the time and labour," says Sir John Forbes, speaking of his *Medical Bibliography*, "bestowed on the following pages, it may be prudent not to speak, lest the actual scantiness of the harvest should appear still more conspicuous in relation to the extent of the workman's toil. We may, however, venture to say this much, that no one who has not actually made the trial is likely to form an accurate estimate of either the one or the other." Dr. Fischer brought to the work all the qualities necessary for a successful issue—education, scientific knowledge, great perseverance, and withal, a love of the thing. The result has been that to him belongs the honour of having compiled the best medical catalogue in the English language. We congratulate him most sincerely on having performed so severe a task in so able a manner—a task which few would have had the courage to undertake, and still fewer the ability to carry through.

S. L.

ART. XIX.—*Reports of American Institutions for the Insane.*

1. *Of the Maine State Hospital, for the year* 1856.
2. *Of the Vermont State Asylum, for the fiscal year* 1855–6.
3. *Of the McLean Asylum, for the year* 1856.
4. *Of the Butler Hospital, for the year* 1856.
5. *Of the Hartford Retreat, for the fiscal year* 1855–6.
6. *Of the King's County Asylum, N. Y., for the fiscal year* 1855–6.

1. IN the report from the Maine Insane Hospital, we are informed that during the year ending on the 30th of November, 1856, "the operations of the Institution have been highly prosperous and successful. A number of patients considerably larger than ever before have enjoyed its benefits. No unusual sickness has visited the inmates, and no case of suicide or homicide has occurred."

	Men.	Women.	Total.
Patients in the hospital Nov. 30, 1855	86	69	155
Admitted in course of the year	85	64	149
Whole number	171	133	304
Discharged, including deaths	67	47	114
Remaining Nov. 30, 1856	105	85	190
Of those discharged, there were cured	29	25	54
Died	14	5	19

Causes of Death.—General paralysis 6; tubercular consumption 4; maniacal exhaustion 3; marasmus 2; peri-pneumonia 1; diarrhœa 1; serous apoplexy 1; epilepsy 1.

"The legislature, at its last session, enacted a law by which the State assumes one dollar per week of the expense of all patients sent to the hospital who are unable to pay their own bills, and have no relations of sufficient means liable by law for the same. Eighty-six unfortunate insane persons have enjoyed the comforts and blessings of the hospital through the provisions of that law, who (many of them at least) would otherwise never have seen the Institution, or, having seen and known its comforts, would have been returned to almshouses—and some, alas! to cages and chains."

A vast many figures have been wasted in the printing of worthless statistics of insanity, yet the number is not small of those which have been employed in the publication of such as are of very considerable value. Some of the most important and reliable information of this kind which has thus been placed before the profession, first appeared in the annual reports from the Maine Hospital, when that institution was under the superintendence of Dr. James Bates. Since Dr. Bates resigned, the tables have gradually left the report until, in the one now before us, the only *figures* giving information in regard to the patients are contained in the following sentence:—

"Since the hospital has been in operation, a period of some sixteen years, there have been 1,708 patients admitted, and 1,518 discharged. Of the latter, 685 recovered; 298 improved; 323 unimproved; and 212 died."

It is not recollected that Dr. Harlow has expressed any opinion, in either of his reports, in regard to statistics. The inference from his practice is, not only that he has no faith in them, as applied to mental disorders, but that, like some other physicians who are, or who have been employed in the same specialty, he fully believes in the *converse* of the proposition, "Figures cannot lie." Hence, in this report, the numbers admitted, discharged, remaining, &c., are all printed in *words*, and not in *figures*. The information formerly imparted by tables of civil condition, age, and causes of insanity, is also conveyed in the same manner; and there the statistical matter stops.

If this method was adopted for the purpose of avoiding errors, by typographical or other mistakes, it has signally failed, as any one may perceive by an examination of the subjoined extracts:—

"We commenced this year with one hundred and fifty-five patients—eighty-six males and sixty-nine females. There have been admitted, during the year, one hundred and forty-nine—eighty-five males and sixty-four females, making a sum total of three hundred and four cases under treatment—one hundred and seventy-one males, and one hundred and thirty-three females.

"One hundred and fourteen have been discharged from the hospital during the year—sixty-seven males and forty-seven females; leaving in the institution, on the 30th of November, 1856, one hundred and ninety patients—one hundred and five males, and eighty-five females."

The final clause of the last sentence, to be consistent with all that precedes it, should read "one hundred and four males and one hundred and six females." Yet, in converting this information into its representative figures, near the beginning of this notice, we have followed the text. Before leaving this subject we must remark—and it is done without any special reference to the opinions or the action of Dr. Harlow—that, of the worthless statistics of insanity, a large proportion are valueless simply because the labour necessary to render them valuable has not been bestowed upon them.

2. At the Vermont Asylum for the Insane—

	Men.	Women.	Total.
No. of patients on the 1st of August, 1855	185	209	394
Admitted in the course of the year . .	80	92	172
Whole number	265	301	566
Discharged	75	84	159
Remaining, August 1, 1856 . . .	190	217	407
Of those discharged, there were cured .			82
Died			38
Admitted since the opening of the asylum			2,565
Discharged recovered			1,209
Died—not stated.			

Dr. Rockwell closes some very appropriate remarks upon the importance of early treatment, as follows:—

"Although it is very desirable that cases of insanity should be placed in some proper asylum in the early stages of the disease, still we would caution those who are interested, not to bring their friends who are afflicted with other diseases, or before their insanity is fully developed. It is no uncommon circumstance to have sent us a case in the early stage of delirium, and while suffering from acute bodily disease. When they reach us they are frequently too feeble to return to their homes. These cases frequently prove fatal, and the death, in many instances, is hastened by the exposure and fatigue of the journey."

He illustrates the injurious effects of the premature removal of patients by the subjoined case:—

"A young married lady had been a few weeks in the asylum, and was rapidly convalescing. When she came she was in a highly excited state, and her mind was filled with the strangest delusions. She had now become more calm. The delusions had nearly all faded away, and she began to be conscious of her situation. But her mind was still in a very weak state, liable to a relapse at any moment, and the greatest caution was necessary to keep from her all extraneous excitement. At this critical period her husband came to see her. He was charged with a common idea that there might be something in his presence that would have a happy influence upon the mind of his wife. We endeavoured to dissuade him from seeing her, but his mind was fully made up to take the risk. He found her much better than he expected, and she seemed better the longer he talked with her. She asked after every member of the family, and said she was glad he had come for her. His mind was not made up to take her away just at present, but she talked so well, and had so clear a recognition of her past situation, that he could not resist the eloquence of her arguments and appeals, and they went home together. The result was anticipated by us. Her mind soon gave way under the rush of old associations

and new excitements, and she was brought back again in a worse condition than at first. It will be a long time before she will be so nearly well again, and it will be fortunate indeed if she is not a confirmed lunatic."

3. The report for 1856 from the McLean Asylum, is the first issued over the signature of Dr. Chauncey Booth, the successor of Dr. Bell. The long experience of Dr. Booth in the treatment of the insane, has given him those professional qualifications for his post, which are essential to the best fulfilment of its attendant duties:—

	Men.	Women.	Total.
Patients in the asylum, Jan. 1, 1856	88	104	192
Admitted in course of the year	87	62	149
Whole number	175	166	341
Discharged, including deaths	81	64	145
Remaining, Dec. 31, 1856	94	102	196
Of those discharged, there were cured	36	32	68
Died	10	9	19
Patients admitted from 1818 to Dec. 31, 1856			4115
Discharged, recovered			1926
Died			465

Of the moral treatment of the insane Dr. Booth writes as follows:—

"Every one who comes in contact with the insane, in whatever capacity, whether by his words, manners, or acts, does something to help or to hinder the success of the moral treatment. * * * This plan of moral agency is progressive. * * * There is no limit to the application of these means; and that institution which fancies it has attained perfection has already begun to retrograde. * * * Every day suggests something new."

In reference to the fact that insanity has apparently increased, *pari passu* with civilization, he says: "This state of things is not a necessary condition of true advancement. If the spirit of free inquiry, and the enterprise engendered by our institutions lead to excesses; if we abuse the power afforded us by our blessings; if the intellect is stimulated to its utmost, while the moral and affectional portion of our nature is neglected; if, in our haste to secure immediate results, we sacrifice the greater boon which a thoughtful consideration would have secured; and if, in all our seekings and searchings, we learn everything else, but remain ignorant of that higher knowledge, the knowledge of ourselves, of the laws which are ordained for the government of our physical and mental organization, we have no right to expect that our civilization will prove a source of unmixed satisfaction and true progress."

"It is asserted that the insane are more numerous in this country than in any in Europe. * * * It would seem as if our form of government would be the most favourable to the best and highest development of our mental and moral powers; and yet this pre-eminence may furnish our great danger. Our system of education, the abundant opportunities for obtaining wealth, and the ingenuity and rivalry which these call into action, the necessity which is felt by so many to engage intensely in politics; the zeal that is manifested in the multiplied forms of religious interests, or in the varied objects of philanthropy; and the ardor which multitudes exhibit for the divers and ever-shifting *isms* and *ologies* universally prevailing; all these, and every other phase of mental effort, tend to excite and produce unhealthy changes in the brain, and thus to prepare the way for insanity, if they do not actually induce it."

After mentioning some of the defects of education, he remarks: "If this abandonment of the young to chance, or to influences which cannot fail to surround them when away from home, continues much longer, the term 'moral insanity' will cease to raise a smile or a sneer, and have a significance in the popular estimation hardly less prominent than it has in the professional mind. Our range of observation need not be extensive to satisfy us of the truth of this position. Reverence for age has become well nigh obsolete; and the younger branches of the community, instead of serving the elders, are most emphatically served by them."

In relation to hereditary predisposition, the two following cases are related: "A young woman who a few years ago was under the care of this institution, was immediately related to no fewer than ten persons who had destroyed themselves; and she, too, died by her own hands."

"A woman, past fifty, of a well-balanced mind, began, about a year since, to exhibit great irritability, and, a few months later, was seized with apoplexy which resulted in insanity. She was immediately brought here, but died soon after, in an attack of apoplexy. Two brothers, a sister, an uncle, and an aunt, and several more distant relatives have terminated their lives in a similar way."

4. The record of changes at the Butler Hospital, is as follows:—

	Men.	Women.	Total.
Patients on the 1st of January, 1856 . .	61	76	137
Admitted	38	21	59
Whole number	99	97	196
Discharged, including deaths	29	24	53
Remaining, Dec. 31st, 1856	70	73	143
Of those discharged, there were cured . .			14
Died			16
Whole number of patients from 1848 to 1856			778
Discharged, recovered			245
Died			148

One of the most interesting portions of this report is that which was suggested by the recent presidential canvass. We extract the principal part of it:—

"It is worthy of remark that while the past year has witnessed a political campaign which, as it regards the intensity of the feelings excited, and the proportion of people actively engaged in it, is almost, if not quite, without precedent, yet not a single admission could be fairly attributed to a political origin. The fact is one of so much importance, as a matter of mental hygiene, that it may be worth our while to consider it a little more closely. For days and weeks and months, whole communities were living under a pressure of excitement that made every other pursuit than that of politics a matter of subordinate interest. At home and abroad, by the fireside and in the market-place, in private interviews and public gatherings, in mass meetings and evening processions, in lectures and sermons, and speeches innumerable, by day and by night, in season and out of season, the predominant feeling was manifested, as if the dearest interests of the individual were staked upon the result. And yet, after all this turmoil, no one, to my knowledge, was made insane by it.

"This kind of experience seems to confirm a conclusion, not entirely unsupported by other considerations, that mere excitement alone is not necessarily a very efficient agent in the production of mental disease. * * * Mental excitement has been too readily regarded as amply sufficient, not only to disturb the healthy balance of the faculties, but give rise to serious and overt disease. In the cases which seem, at first sight, to warrant this conclusion, there will be generally found, upon closer inspection, some additional element by which the normal effect of simple excitement has been more or less modified. It may be accomplished by an utter disregard of the plainest rules for the preservation of health. Food is taken irregularly, and the functions of the stomach are disordered; the body, while improperly clad, is exposed to atmospherical changes, and thus the sympathy between the skin and the lungs is deranged; the hours that should be given to repose are surrendered to the all-absorbing topic, and for want of the blessed influences which 'tired nature's sweet restorer' diffuses over the whole system, animal and organic, there occurs a morbid irritability that deepens the impression made by every adverse incident. Habits of daily exercise and recreation which have become essential to the physical welfare, are entirely abandoned, and the brain is no longer visited by the invigorating influences that radiate from the muscular system. Any one of these incidents may impart to the mental excitement an unhealthy cha-

racter, and convert it into actual disease. It must be considered, also, that on these occasions of prevalent excitement, all those exercises of the mind which are promotive of peace, cheerfulness, and serenity; that active interest in the welfare of others which withdraws one from a too steady devotion to selfish objects; and especially those glimpses of a higher and brighter sphere, above the agitations of flesh and sense,—all these may be replaced by a complete absorption in the thought or emotion that holds possession of the individual."

Hereditary predisposition is also mentioned as one of the collateral elements which may convert the excitement into disease; and the fact is stated that excitement is less liable to be injurious if the emotions which it excites are pleasing than if they are depressing.

"Bearing in mind these views, we may readily see why the mental health sustains so little harm generally from intense political excitement. It seldom affects a person's habits of living. He takes his meals and exercise as regularly as ever, and with as keen a relish; his sleep is sound and his conscience quiet; his sympathies are not entirely silenced; he acknowledges the claim of his neighbour to a helping hand; and his eye and ear are not utterly closed to what is passing around him. The danger he apprehends, the good he seeks, are somewhat remote; and upon few comparatively of those who mingle in political strife does the result have any immediate personal bearing. Their business, their fortune, their social position, their domestic affections, remain the same, whether the favourite candidate succeed or fail. The emotions which spring from the joy of success, or the pain of failure, seldom come from the depths of the soul, and cannot withstand the intrusion of other sentiments more nearly connected with the ordinary experience."

Yet it is not intended to convey the idea that political excitement is harmless. It is believed to be a part of that general mental activity of the age which promotes disorders of the mind.

Dr. Ray now proceeds to show the difference between religious and political excitement. We extract a part of his remarks:—

"The greater intensity of feeling produced by the former (religious excitement) is to be attributed to the superior magnitude of the interests at stake. They are no holiday matter which one may attend to or neglect without materially affecting his immediate welfare. They involve nothing less than his future destiny, and are well calculated, if anything is, to produce serious thought, if not overwhelming emotion. They also include an element of personal danger sufficient to excite apprehensions that seldom accompany the gloomiest forebodings of national trouble. A sense of responsibility, quickened by the consciousness of past deficiencies and neglected opportunities, also helps to sharpen sensibilities already alive and glowing with excitement. The attention being thus thoroughly aroused, the deepest concern and the strongest emotion would seem to be indicative of the healthiest condition of mind. And so indeed it would be regarded in a moral point of view; but we are made up of body and mind, and the proper question is not what experiences are most commendable abstractly considered, but what are most conducive to the welfare and development of both. The activity of each must be in exact relation to the power of the other, if we would avoid an imperfect and morbid result. In certain exigencies, and especially when controlled by that sympathy which unites people who are possessed by a common emotion, the will is apt to impose upon the bodily powers an amount of effort beyond the healthy limit of endurance. The intention is good and the object desirable; but nevertheless an organic law is infringed, and we pay the penalty in the shape of disorder and disease. The successive steps that lead to this result are obvious enough to the careful observer. In the pursuit of an object of paramount importance, other objects are apt to be degraded below their proper level; the healthy balance of the mental functions, as well as the various duties of life, is deranged; recreation and exercise are neglected; food is taken irregularly; sleep becomes less and less sound and refreshing; and the whole nervous system is strained to its utmost power of tension. Under such circumstances, the individual is prepared for the invasion of any disease to which he may be predis-

posed; and hence it happens that insanity is so common an accompaniment of what is called religious excitement.

"Let me not be misunderstood. I have but poorly expressed my ideas if I have conveyed the impression that, in this matter, religion, or the religious sentiment, is at fault. On the contrary I believe—and it is in some measure the result of considerable observation of various psychological states—that, in this age of fast living, nothing can be relied upon more surely for preserving the healthy balance of the mental faculties than an earnest, practical conviction of the great truths of Christianity. But in striving to obtain this conviction so as to make it a guiding principle of life, as in the pursuit of any other commendable object, the best designs may be imperfectly executed, and the best intentions frustrated by reason of that all-pervading element of human concerns, human infirmity."

So much for the abstract and the theoretical of this report. Let us turn for a moment to the concrete and the practical. It having been determined to introduce into the hospital the apparatus for heating by steam and ventilating by mechanical power, "no time was lost in discussing plans for raising the requisite funds" ($15,000), but the whole amount "was subscribed upon the spot" by the President and the Board of Trustees of the Institution.

5. In the report of the Hartford *Retreat for the Insane*, for the fiscal year terminating with the 31st of March, 1856, Dr. Butler gives the following numerical results:—

	Men.	Women.	Total.
Patients in the Retreat March 31, 1855 .	85	108	193
Admitted in course of the year . . .	70	87	157
Whole number " " . . .	155	195	350
Discharged, including deaths . . .	57	96	153
Remaining March 31, 1856 . . .	98	99	197
Of those discharged, there were cured .	18	41	59
Died	12	14	26

Deaths from exhaustion 5; diarrhœa 4; epilepsy 4; consumption 4; general paralysis 2; apoplexy 1; old age 1; general debility 1; "disease of the brain" 1; suicide 1; erysipelas 1; purpura 1. Whole number admitted since 1824, 2,961. Discharged cured 1,463. Died 308.

The great distinguishing characteristic of the institutions for the insane of the present day, is an enlightened moral treatment. That treatment is based upon the law of kindness; upon those better emotions and feelings of our nature which, whether named benevolence or philanthropy, induce us to act towards others with a single eye to their best welfare. But what is true kindness towards the insane? Does it consist in ministering to all their physical necessities and comforts, and permitting them to do as they please? By no means. Is he a judicious, a truly kind parent, who feeds, and clothes, and shelters, and educates, intellectually, his children, and yet suffers the baser qualities of their moral nature to run riot, unchecked and unsubdued? Shall the wayward child not be subjected to discipline, the froward curbed, the passionate taught to place a rein upon his anger? Shall the boy whose unreasonable demands have met with a refusal, whose desires have been ungratified, or whose wilful and malevolent nature incites him to the acts, be permitted to convert the house into a bedlam, and to maltreat, by both word and act, every person who comes into his presence? Surely not. True kindness, the ultimate and the best interests of the boy, demand that such conduct be not permitted. But the insane, as was many years ago remarked by Esquirol, and as must have been observed by every person long engaged in the management of them, are very much like children. In their conduct, their feelings, their motives to action, and their reasons or motives for repression of action, there is a striking similarity. The number of the insane who have not generally the power to control their actions, is but very limited. To teach them to exercise that control is an essential part of their moral treatment. The sooner that power is called forth, the sooner will they be restored to mental soundness.

These remarks have been suggested by a portion of Dr. Butler's report, from which we make an extract.

"That asylum for the insane is poorly cared for where the wants of the body are alone abundantly provided for, while the cravings of the heart are left in hunger and nakedness. Far better, in my view, to banish all other remedies from the wards of such an asylum, than to leave them destitute of that personal, practical sympathy which, carrying to their hearts a conviction of its entire sincerity and fellow-feeling, is daily active among them in cultivating an equable healthy tone of feeling. Nor are cheerful influences alone demanded. Despondency is far from being the only unhealthy element developed. Undue excitability, and the more coarse and violent elements of our nature are here let loose, ready to run in wildest riot. Hence, in establishing the discipline of such a household, no reckless indulgence of passion, or of delusion, capable of restraint, is to be allowed to violate its decorum, without such penalties of reproof, banishment, or seclusion, as the good sense of every witness will approve. The influence of a public sentiment which has been carefully and thoroughly developed, and is kindly, yet decidedly applied, is of far greater efficiency in promoting good order and discipline than most can imagine. This position may be illustrated by a single example. A violent outbreak, both of action and language, a short time since, compelled me to remove a young lady to one of the lower departments of our system of classification, and thus to deprive her of the privilege of refined society and pleasant associations; I was soon recalled at her urgent request, and found her in a paroxysm of tears. She gave me the most earnest promises of future self-command and lady-like deportment. I restored her at once to her former position, jocosely introducing her to her old associates as an estimable young friend of mine from the country, in whom I had great confidence. The lesson, benefiting others beside herself, required no repetition. Its influence continued until entire restoration to health speedily removed her from my care. Those whose frequent visits to our house have afforded them a good opportunity of judging of the tone of feeling of our family, can, perhaps, best appreciate the merits of such a system of discipline."

Although not among the "frequent" visitors at the Retreat, yet we were there, a few months ago, a sufficient time to enable us to form an opinion in regard to the "tone of feeling" among the patients. Briefly, then, we may assert that we have never visited an institution of the kind in which there was more apparent cheerfulness and content; one in which the whole body of patients appeared in a greater degree to have cast aside the individuality, reserve, and exclusiveness of strangers, and to have adopted the characteristics of a united family. Dr. Butler has been untiring in his efforts for the attainment of this end, and continues so, for its perpetuation. The patients are surrounded by conditions and by agents ministering to their physical comforts; the walls of the corridors and the rooms are decorated with a multitude of the better style of pictures; social reunions and *extempore* dancing parties are of frequent occurrence; and the Doctor devotes much of his time to conversation with the inmates of all classes, as he makes his daily visits to their several departments.

6. At the Lunatic Asylum of King's County, N. Y.—

	Men.	Women.	Total.
Number of patients on the 31st of July, 1855,	75	110	185
Admitted in the course of the year . .	65	80	145
Whole number	140	190	330
Discharged, including deaths . . .	60	76	136
Remaining, July 31st, 1856	65[1]	119[1]	194
Of the discharged, there were cured . .	39	54	93
Died	7	9	16

[1] These are the numbers given in the report. To correspond with the preceding figures, and to make the sum 194, they should be 80 and 114, instead of 65 and 119.

"The general health of the patients has been remarkably good," says Dr. Baiseley, "notwithstanding the crowded condition of our buildings. There are in the Asylum, at the present time, fifty female patients more than can be accommodated as they should be; nor can they be classified in such a manner as would be beneficial to them." He therefore recommends the building of another wing. P. E.

ART. XX.—*Indigenous Races of the Earth; or, New Chapters of Ethnological Inquiry, including Monographs on Special Departments of Philology, Iconography, Cranioscopy, Palæontology, Pathology, Archæology, Comparative Geography, and Natural History; contributed by* ALFRED MAURY, Bibliothécaire de l'Institut de France, Secrétaire Général de la Société de Géographie de Paris, &c.; FRANCIS PULSZKY, of Lubocz and Cselfalva, Fellow of the Hungarian Academy, &c.; and J. AITKEN MEIGS, M. D., Professor of the Institutes of Medicine in the Philadelphia College of Medicine, Librarian of the Academy of Natural Sciences of Philada., &c. [with communications from Prof. Jos. Leidy, M. D., and Prof. L. Agassiz, LL. D.]; presenting fresh investigations, documents, and materials; by J. C. NOTT, M. D., &c., and GEO. R. GLIDDON, &c. &c. Philadelphia: J. B. Lippincott & Co., 1857.

"ETHNOLOGY may have been regarded as a series of fanciful and, probably, futile inquiries, leading to no very definite ends, and the ethnologist as a sort of harmless visionary, led hither and thither by trifling indications, and exciting more smiles than looks of satisfaction. In such a region, hypotheses have been very prolific, and the pertinacity of their inventors has usually been in the inverse ratio to their stability and the number of the facts on which they have built them. Great learning has often been expended, even by men of sterling merit, upon investigations into the origin, migrations, and settlements of early nations, without any fixed principles or sound philosophy to guide or to support the inquiries entered upon. * * * * * * Such vague lucubrations may be very fascinating, but are chiefly to be tolerated on the principle of the old French maxim, 'Du choc des idées jaillit la lumière.'"

"It is the misfortune of new sciences that they afford none of those necessary checks on individual folly and presumption without which learning is but a despotic creed, or a clash of jarring opinions."

If such language has been, not long since, used by two very distinguished ethnologists of the "diversitarian" school, J. Barnard Davis and Luke Burke, we need not hesitate to accept its applicability, to some extent, to ethnology now, when we read such works as the *Types of Man* and *Indigenous Races of the Earth*. These, both, contain much information, of which a sufficient proportion is new; they promise, in their titles, very great results; not only the mountains, but the deserts and plains, the mounds and the monuments "parturiunt;" but, there is brought forth, at last, only *hypothesis*.

We have been, upon the whole, less disappointed with the present work than with the *Types of Man*. It contains, like the latter, some articles marked by great ability as well as research; and, in one or two of them at least, displays a more cautious spirit than the *Types*, so as to make it more worthy of scientific attention.

The pages of a medical journal, however, afford but little space for the discussion of such subjects, whatever may be their interest. We shall be obliged, therefore, to content ourselves with some very summary remarks, which, although not hastily conceived, it will be impossible here fully to elucidate, or to support with such references as they may seem to require.

After a brief communication from Lieut. Habersham, U. S. N., in regard to the "hairy Kubiles" and "red men of Formosa," a short letter is introduced, from Prof. Agassiz. This contains a reassertion of his confidence in the theory of "Natural Provinces," and its applicability to man, a brief allusion to the complexional relations of monkeys and men, and a protest against the relia-

bility of philological evidence of race-derivation, upon grounds of zoological analogy. We might remark, in passing, that the "identity of colour of the Malays and Orangs," and of the "Hylobates with the Negrillos and Telingans" (a very interesting fact), would seem to be easily accounted for, by the similar influence of climate and local conditions upon different species, without proving, or even suggesting, *anything* in regard to the origin of either.

The theory of "Natural Provinces" is the most consistent proposed upon the side of the Polygenists. It advances the "coincidence of the geographical distribution of the human races with that of animals," asserting that "the adaptation of different races of men to different parts of the world must have been intentional, as well as that of other beings; that men were primitively located in the various parts of the world they inhabit." It is asked, in the words of Agassiz himself (*Types of Man*, p. 78), "how could it be supposed that man alone would assume new peculiarities and features so different from his primitive characteristics, whilst the animals and plants circumscribed within the same limits would continue to preserve their natural relations to the fauna and flora of other parts of the world?" We may answer this, respectfully, with the simple inquiry—how is it that the distinguished author of this question is himself a noble "exotic" from a distant land, and that he is in the midst of a nation made up of the most varied elements, *all of foreign races*, yet which is increasing (as Wynne's late statistics show), irrespective of immigration, faster than any other on the globe? Nor is such migration confined at all to the *Japetidæ*. The negroes, on the same continent, have, according to Dr. Nott, in the Southern United States "assimilated readily to our climate," exhibiting remarkable instances of longevity; the Jews pervade nearly every longitude and latitude; and the Zingarri, or Gypsies, wander all over the world. *Degrees* of *mobility* of species, therefore, must be acknowledged, as in the case, also, of our domestic animals and cultivated plants; and, this being the case, *how are we to limit this* short of the possibility of the province of *Man* reaching from the central table lands of Asia to Iceland and to Terra del Fuego?

The theory of *Specific Centres*, of Prof. Edward Forbes, may be believed to be more in accordance with the facts of natural history. It maintains the probable "origination of each species from a *unique stock*, or prototype, consisting of a single being, or pair of beings, according as would be required for propagation." This, with the law of *variability*, under known and unknown causes, greater in some species than in others, and greatest in man and the domestic animals, will account for all that we know of human distribution, especially in view of its well established *gradations*.

To pass on. We have, next, in the book before us, a letter from Prof. Leidy. It exemplifies the fact that the *severest* observer must, occasionally, indulge in theory; while the success of its excursions is, perhaps, not facilitated by desuetude. In the space of four pages, we are met by not less than three hypotheses, in regard to the "traditions of the deluge," to the cotemporaneousness of the "red man with the mastodon," and to the probable *successive* creations of human races—to establish either of which would require the overturn of much supposed positive experience.

We may, in this place, allude to a very philosophical definition of *species* by Dr. Leidy, quoted in another part of the volume. "A species is a mere convenient word with which naturalists empirically designate groups of organized beings, possessing characters of comparative constancy, as far as historic experience has guided them in giving due weight to such constancy." This is much safer than Morton's "*primordial* organic form." De Candolle and De Blainville long since recognized the hypothetical nature of the ordinary definition, and its resulting inconvenience, which is especially evident in discussions upon the diversities and origin of mankind.

Alfred Maury's essay on the *Distribution and Classification of Tongues, &c.*, is an admirably written (hardly so well translated) digest of comparative philology, in which, after the assumption of the essential unity of the human *mind*, so clear an account is given of the manner in which we may trace almost every known tongue to an Asiatic relationship, that, but for an occasional implication, and the concluding expressions, we should have supposed

it to have been written by as "monogenistic" an author as Latham, or Johnes, or Schele De Vere.

Iconographic Researches on Human Races and their Art follows, by Francis Pulszky. This is, also, ably written, and with research, although hardly so complete in its method as the preceding. Races are classified, by Pulszky, as "artistic" or "non-artistic;" this characteristic being independent of their general mental culture or capacity.

The Cranial Characteristics of the Races of Men, by J. Aitken Meigs, is the next paper. This occupies one hundred and fifty pages, and is an elaborate and complete monograph on cranioscopy in all its relations, as brought up to the science of the present time. It is founded chiefly on the Mortonian collection of crania in the Philada. Academy of Natural Sciences, of which a new catalogue has recently been issued by Dr. Meigs. The tone of this paper is scientific, "carefully and designedly," to use the author's own language, "abstaining from the expression of any opinion upon the prematurely, and, perhaps, in the present state of our knowledge, unwisely mooted questions of the origin and primitive affiliations of man." The two most important, in our view, of the conclusions to which Dr. Meigs has been led by his extensive survey of craniology, are, that a regular system of *gradation* seems to underlie and harmonize the various cranial forms of the human family, and, that these forms display an *inseparable connection with the physics of the globe.*

Dr. Nott's views upon "acclimation, &c.," are familiar, to some extent, to the readers of this Journal. That medical climatology is, even with his investigations, in a yet imperfect state, and unfitted to afford a basis for ethnological conclusions, will appear from the facts, that the so-called "autochthonous" American Indians are now disappearing at a more rapid rate of mortality than that of any other race (though all others are imported) upon the continent;[1] and that, although Dr. Dowler is quoted as concluding that yellow fever is *not an African* disease, the negro race is the *only one* which enjoys congenital immunity from it. We have no room for criticism upon this paper, although it invites it, especially by the boldness of the conclusions which it deduces from premises which are rather less obvious to some of his readers at least than to the author himself.

With regard to the contributions of G. R. Gliddon, which fill the last two hundred and fifty pages of this volume, we find it somewhat difficult to continue in the same strain of remark. Their manner is the reverse of the scientific, and their matter much less philosophical than that of his colleagues in the work. Egypt is the favourite topic of his declamation, which wanders, however, over many others, without restraint, and, apparently, almost without object.

Upon the whole, the conclusion to which, so far, the ablest and most zealous of naturalists, physiologists, philologists, and archæologists, have, in large majority, arrived, does not seem, notwithstanding the title of this work, to have been altogether overturned by it. Entering upon its examination with the most absolute impartiality, we must, still, be content to believe, with Linnæus, Buffon, Cuvier, Blumenbach, Lawrence, Prichard, Lyell, Forbes, Müller, Owen, Carpenter, Flourens, Draper, Pickering, Latham, Lepsius, Bunsen, and Humboldt, that man constitutes but a single species. Yet, the ethnology of his varieties must ever be a most important subject—the crowning department of physiology, and intimately related to psychology, history, and political science. If its votaries will but cease confining their efforts to the establishment of any particular dogma, whether of unity or diversity, its advances will be more rapid, and its conquests more substantial, than any of those which the editors of the present work have, as yet, in our view, accomplished.

The publishers of the *Indigenous Races*, by its typographical excellence, and plenitude of illustration, have done all in their power to satisfy the expectations of the authors, and of their large list of subscribers. H. H.

[1] Vide Army Med. Statistics, 1856.

ART. XXI.—*Annual Report of the Physician-in-Chief of the Marine Hospital at Quarantine.* Presented to the Legislature of New York, February 4, 1857. 8vo. pp. 64: Albany, 1857.

THE report of Dr. Harris, Physician of the Marine Hospital of New York, is chiefly interesting from the history it presents of the yellow fever as it prevailed during the summer of 1856, at different points along the shores of New York Bay.

The first case of the disease was admitted into the Marine Hospital, Staten Island, on the 18th of June, from on board of a vessel that had just arrived from St. Jago de Cuba, where yellow fever was beginning to prevail when she left. Between this date and the evening of the sixth of July, four other vessels, all having the fever on board, arrived from Havana, where the disease was epidemic at the period of their departure. During the month of July, a number of other vessels, with the disease on board, arrived from infected ports, and were detained at the quarantine station. It was in persons from on board of those vessels, or who were engaged in unloading their cargoes, or had visited them, or been in their immediate vicinity, that all the first cases of yellow fever in the vicinity of New York occurred.

Previous to the 15th of July, several of the infected vessels lying at quarantine had discharged cargo upon lighters under quarantine order. Up to that date twenty-seven yellow fever vessels had arrived; and of this number, ten or more were lying at anchor nearly opposite the Marine Hospital, or at Gravesend Bay. One had discharged a portion of her cargo, and, as subsequently appeared, fourteen bales of filthy rags from on board had been stored in an open shed on the U. S. Government dock at quarantine.

While lying at quarantine those vessels had destroyed, or cast into the bay, large quantities of refuse matter, decaying fruits, old bedding, etc., and all such matters as floated on the water were carried in directions and to localities which became, subsequently, the lurking places of pestilence.

"The total number of seamen admitted to Marine Hospital with yellow fever, immediately upon the arrival of the vessels from sickly ports, was seventy-two, and these were received from thirty-three vessels. The number of cases received, subsequently, from the same vessels was fourteen, from five of the vessels. Besides these, there were fifteen seamen admitted with yellow fever from nine other vessels arriving from infected ports, and upon which there was no sickness at the time of arrival at quarantine. Thus, the total number of seamen admitted from vessels presumed to be infected was one hundred and one, and these arrived on forty-four different vessels. Besides this number, six other seamen having yellow fever were received from the city who had contracted the malady from sources unknown. But as we shall have occasion to notice, in another place, those persons had been peculiarly exposed in certain localities about the city, where there is good reason to believe that the infection of yellow fever existed."

Previously to July 22, five cases of yellow fever had occurred on the beach 1¼ mile south of the hospital, and near Townsend's dock; and six cases, of which three died with black vomit, on the Long Island shore directly opposite the quarantine grounds, between the 13th and 21st of July. From the latter date the malady extended fearfully until it had visited nearly every dwelling on the shore of the beautiful Bay Ridge; thirty cases, and fifteen deaths having occurred in that district previously to August 1.

A chronological summary is presented in the report of all the cases of yellow fever that occurred, during the summer and autumn of 1856, in the port and vicinity of New York. In this are recorded five hundred and thirty-five cases, three cases being added that occurred among patients convalescent from other diseases in the Marine Hospital; it gives a total of five hundred and thirty-eight well-authenticated cases, as ascertained by the most rigid investigation. Of these, more than one-third died of black vomit. Of the twenty-nine cases recorded as occurring in Brooklyn, seven were persons resident in the eastern

district of that city, and twenty-two in the western district, or the City of Brooklyn proper. The origin of a large proportion of these cases, we are assured, was traced directly to infected places and infected goods.

The first case of yellow fever occurred among the garrison on Governor's Island, on July 29, and the malady continued to prevail quite steadily until the 10th of September. Though a very large number of cases occurred in that military command, the victims were selected exclusively from the south battery, which has a southerly and easterly exposure, facing, as it were, the quarantine ground, and all those portions of the bay shores which, with propriety, might be termed the great seat of infection. The officers of the command at Governor's Island assured the author of the report that no *material* cause of infection was conveyed to or existed upon the island.

"The first evidence of the presence of yellow fever in an endemic form in the vicinity of the Marine Hospital was presented July 22, when two cases of the malady occurred among the residents. All access to the grounds was immediately cut off, as far as the physician in charge considered he had legal authority to prevent access to the establishment, and the health of the employees was carefully guarded. The lodgings of the workmen were at once removed to the highest elevation on the western section of the grounds. But the presence of a large company of stevedores lodging in the government store every night, and uneasy of the restraint imposed by the health officer's bonds, made it necessary for the safety of the establishment, as well as the community beyond the walls, that many of our men should serve on the night patrol. This was a perilous service, in which several sickened.

"A very large amount of infected clothing from the sick, and bedding, etc., from infected vessels, were being constantly received and washed at the hospital wash-house, which must be regarded as furnishing a favourable nidus for the infection. Yet that building did not appear to be a source of general infection, as its upper rooms were nightly filled with the stevedores who were unlading vessels, and of whom none that lodged there suffered from the fever.

"From the fact that six persons who were employed at, or spent much time in the gate-keeper's lodge, sickened with the fever at an early date, and, from various other circumstances, we were led to conclude that the infected district within the inclosure probably extended in a narrow zone by the water side from the south to the north wall. West of that section the ground rises to a considerable elevation, and is terraced. None of the persons who remained permanently beyond the first terrace suffered from the fever until the cool autumnal nights appeared, when several very severe cases occurred in the wards and dormitories on the hill.

"Many of those who performed night service seemed to be so saturated with the poison of the malady that, when they yielded to its deadly power, they advanced very rapidly to the stage of black vomit. Indeed, from the outset of the endemic, the fever among the employees was observed to be of the most malignant character—black vomit and hemorrhages occurring in many cases as early as the thirty-sixth hour.

"The total number of officers and employees who suffered from the fever was twenty-five, of whom five died. Besides these, there were eight other cases of the malady among residents.

"Though the stevedores engaged in unlading infected cargoes began to suffer from the fever as early as July 10, the pestilence had not gained any foothold until July 17, when three cases occurred at the narrows, near Clifton Dock, Staten Island. All the cases are readily accounted for without requiring proof of any material fomites in or about the dwellings of those who became the victims of the fever. But it is known that considerable refuse material from vessels in quarantine was floated to the Staten Island beach by every flood tide. Such was particularly true of the section north of the quarantine walls, and at every ebbing tide refuse materials floated and lodged upon the beach near Clifton, where the fever first became endemic on Staten Island. Twenty-nine stevedores, resident on the island, contracted the fever, of whom all but one were brought into the hospital. In addition to these, five stevedores sick with the disease were admitted from the city.

"Three fatal cases of yellow fever occurred previous to July 20, on the Long Island beach, directly opposite to the Marine Hospital; and previous to July 27, fourteen cases had occurred, ten of which proved fatal."

"The first two cases occurred on Mr. Hunt's place, a little below Greenwood Cemetery, and about 300 paces off the beach. The two men were bathing in the bay, Saturday evening, July 11, and examined a quantity of straw which had been left on the beach by the receding tide, and requested their employer, Mr. Bergen, to allow them to remove the straw to the farm yard. The following Monday, one of these men was attacked with all the symptoms of yellow fever, and the other man was attacked on the succeeding day. Both died with black vomit. These men occupied a dormitory in an out-house, and, after they had died, two other labourers were hired, lodged in the same room, and successively sickened and died with black vomit."

"Mr. M. Bergen was quite ill. He allowed nothing brought from the shore to his dwelling, and was on board no vessel; but his residence was very near to, if not situated in the midst of a highly infected district."

On the beach, at the foot of 46th St., lived a Frenchman, a *chiffonier*, who had been very busy gathering and drying refuse clothing, etc., which he picked up along the shore. He sickened July 25, and died on the twenty-eighth. Dr. Harris thinks it probable that in this manner the district in that vicinity became infected.

July 26, Chandler White, Esq., was attacked with the fever, and died of black vomit on the 8th of August. On the 28th of July, five cases of fever occurred in four families near the Bay Ridge dock, north of Mr. White's, and of these, three soon died with black vomit.

"August 1, the pestilence set its fatal seal on the Fort Hamilton district, a mile south of Mr. White's residence, and near to the fort. On that day died General Stanton, of the U. S. Army, and as his mansion was situated on an elevation which looked out directly upon Gravesend Bay, and was surrounded on the north and west by a dense grove, it was presumed that the deadly infection had been wafted to his delightful residence by southerly winds which swept over the infected shipping at Gravesend. Previously to General Stanton's death, a large number of vessels had discharged cargo at that anchorage for infected shipping."

"On the day of General Stanton's death two soldiers in the Redout were attacked with the fever, and up to August 8th, twelve cases had occurred in the fort. The history of these cases was such as to convince the intelligent and distinguished physician of the garrison at Fort Hamilton, that the infection had reached that stronghold by atmospheric agency only."

"Thirty-two cases of yellow fever occurred among the command at Fort Hamilton, besides the case of its distinguished physician, Dr. J. H. Bailey, whose unwearied labours for the relief of the pestilence-stricken population beyond, as well as those within the fort, were terminated by a severe attack, from which he finally recovered.

"It will be observed that we have noted three distinct and nearly equidistant points at which yellow fever made its appearance on the Long Island shore, between Gravesend Bay and Gowanus, a distance of nearly four miles. Subsequently, viz., about the 10th of September, the fever made its appearance in one house near Patridge's mill, near Coney Island. But those who became its victims are believed to have handled materials that floated on shore from the vessels. As regards the nature and mode of the infection received at the first-named three points, there can be no doubt that at the two most southerly, the cause and mode of access were *atmospheric*, while at the first, near Gowanus, it may have been by *material fomites*."

"Fifty cases of yellow fever were admitted to the Marine Hospital from the city of New York. Eight of these cases were traceable directly to Brooklyn, and the origin of nearly all the others was traced directly to vessels which were believed to be infected. The only exceptions, besides those which we have previously mentioned, were: 1. Several seamen and one Italian organ-grinder, from Water Street. 2. Six persons from the immediate vicinity of pier No. 11, and Albany St., North River. 3. Two cases from the water side

near the foot of 31st and 28th Streets, North River. All these were strongly marked and unmistakable cases of yellow fever, several of them terminating in black vomit, and there can be no doubt that in all of them the infection was received at certain well known localities by the river side.

"Most of the cases of yellow fever which remained and were treated in the city were traced to the infected shipping at quarantine. The cases admitted into the hospital and traced to Brooklyn furnished no proof that any other section of that city was infected than that which is situated between the Atlantic Dock and the South Ferry. There were three cases, however, the history of which afforded evidence that, by some means, the infection of the fever had reached some section near the Wallabout. But the etiology of these, like that of the cases received from the vicinity of 31st Street, North River, will, perhaps, remain buried in obscurity. They occurred late in the season, and may have originated from sources that did not expose others to danger."

"September 4, four persons, the remaining members of a family in the 15th ward, New York, were admitted to the Marine Hospital, two of them being strongly marked cases of yellow fever. The mother and son had died just previously; the son's illness being yellow fever with black vomit. This family had, for the purpose of sea-bathing, been residing for two or three weeks at Craven Point, Bergen, N. J. The house they occupied was so situated that at the highest tides it was nearly surrounded by water, and frequently there was much refuse material, &c., left on the beach by the receding tide. The owner of the dwelling suddenly sickened, and, after but a few days' illness, died. His widow subsequently became ill with like symptoms, but recovered. During the brief interim between these events, the family from New York returned to their residence in the city, where, in the course of four days, the first case of yellow fever occurred in the circle that had recently left their pleasant residence by the seaside.

"July 23, a man from Bergen Shore, N. J., was admitted to the New York City Hospital with yellow fever. He was a farm servant, and had not been absent from the farm for a long period. He died July 24th."

Other cases occurred in Jersey City, and at other points upon the shore of New Jersey, of which no accurate accounts have been obtained.

Dr. Harris refers next to what he considers the tidal agencies connected with the production and spread of yellow fever during the period of which he treats, and the meteorological phenomena of the season.

"The flood-tide," he remarks, "has great force in passing the Narrows, and in the vicinity of Staten Island shore near Quarantine. Floating materials are conveyed with great facility and certainty towards Craven Point and Jersey City, as well as up both the North and the East Rivers. The receding or ebbing tide returns such of those floating materials as do not find lodgment about the shores just mentioned, back towards the Narrows, or down the Kill Van Kull towards Newark Bay, the greater force being in the direction of Yellow Hook, and Fort Hamilton, and Clifton. The entire beach of Yellow Hook is the grand lodging-place for the refuse floating materials that are borne on the receding tides of the upper bay. In that locality the pestilence first made its appearance, prevailed longest, and was most malignant.

"There can be no doubt that the most active cause of the pestilence which more than decimated the entire population of the water side in the last-mentioned locality was from the accumulation of infected materials floated from the vessels in quarantine. We have already noticed the extension of the fever to New Jersey shore by the same agency, and it is rational to presume that the cases of fever which were admitted from the upper districts of New York and Brooklyn received the infection from a similar cause.

"The agency of the tides, therefore, must be regarded as one of the most important and active in diffusing yellow fever from the present quarantine anchorage, especially as the distance to, and the location of the favourable points for retaining floating materials is such, as to give the greatest facilities for a ready deposit of such *fomites*."

The meteorological register given in the report was kept, as directed by Government, at Fort Hamilton, by Dr. J. H. Bailey, U. S. Army.

"I will only refer in this place," says Dr. H., "to one striking fact noted in the register. Between the 21st and the 30th of July the wind was continually from the southwest during the afternoon of each day, while the temperature ranged unusually high (from 80.66, to 69.66, daily mean-average 76.3), and was accompanied with a corresponding degree of humidity of the atmosphere.

"It will be recollected, that on the 29th of July, yellow fever was announced as endemic at the fort and its vicinity; and at the same time it was raging most fearfully at quarantine. The same date also marks the commencement of the endemic on Governor's Island."

From a careful examination of all the facts contained in the report of Dr. Harris, of which we have thus given a very brief outline; which facts appear to us to have been collected with much care, and faithfully recorded, we think that some important conclusions may be deduced.

First, the danger of engendering malignant disease, by detaining a large number of infected vessels upon the same anchorage ground, and in close proximity to each other; especially during a season when the temperature and hygrometric condition of the atmosphere are such as are adapted to promote the development of disease.

Second, that the infectious air productive of yellow fever may be carried by the wind to some distance from the locality at which it is generated, so as to produce the disease in places where we have no reason to suppose that it would otherwise have occurred.

Third, that the danger of the production of disease by an aërial poison conveyed by currents of air from infected ships or localities will be always in direct proportion to the extent to which the healthy condition of the atmosphere of places lying in the course of such currents is already impaired by local causes, and the proximity of the places to the source of infection.

Fourth, that none of the facts contained in the report before us, prove positively that filthy clothing, bedding, etc., from on board of infected vessels, or the damaged and refuse materials and garbage cast into the water from such vessels, and conveyed by the tide and left by it upon the neighbouring shores, act as material fomities of yellow fever, in the correct sense of the term; or that contact with them alone is capable of communicating the disease. There can be no doubt, however, that the accumulation of any or all of the articles and materials referred to, beneath a hot sun, and amid a humid and stagnant atmosphere will be liable to produce a condition of things well adapted to favour the outbreak of yellow fever, increase the number of its subjects, and augment its malignancy, when all the other elements for its production are present, whatever these elements may be; whether they consist of some unknown endemic or epidemic influence, or the infected air from the holds of vessels recently arrived from a yellow fever locality.

Fifth, there is not a single fact reported by Dr. H. that countenances, in the slightest degree, the supposition of the spread of yellow fever by personal contagion; on the contrary, the entire history of the disease as presented to us in his report, would seem to be directly adverse to such supposition.

D. F. C.

ART. XXII.—*Report on the Origin of the Yellow Fever in Norfolk during the Summer of* 1855, *made to the City Councils by a Committee of Physicians.* 8vo. pp. 44. Richmond, Va., 1857.

THE report of the committee appointed by the Councils of Norfolk, Va., to investigate the cause and origin of the yellow fever which prevailed in that city during the summer of 1855, presents a very full and apparently careful history of the epidemic, from the occurrence of the first case until the disease had spread over nearly every portion of the place.

The report commences with a sketch of the position and topography of Nor-

folk, and a succinct account of the previous epidemics of yellow fever that have occurred there.

The great object of the committee is to show that on no occasion, but more especially in 1855, was the yellow fever generated in that city from local causes; that the state of the weather, in respect to its temperature and hygrometric condition, was not such, either preceding or at the period of the outbreak of the disease in the latter year, as is usually considered indispensable to the production of yellow fever; that, in the condition of the city, as to general uncleanliness, unpaved streets, the accumulation of putrefying materials in the infected localities, recent excavations causing extensive disturbances of soil, or the existence of any other local cause, was there anything by which the origin of the disease there could be accounted for.

The committee refer to the steamer Ben Franklin, which arrived from St. Thomas on the 6th of June, with yellow fever on board, as the source from which the disease originated, and from whence it spread finally over the entire town. Not, however, by contagion—not from the disease being communicated from the sick to the well—but by the diffusion in the air, and the conveyance by currents of wind of a material organic poison capable of rapid multiplication in a congenial soil or atmosphere.

Taking all the leading facts as presented by the committee as correct—and there is no reason whatever to dispute their entire accuracy—and admitting that all the facts connected with the origin and spread of yellow fever in Norfolk during the summer of 1855 have been fairly reported by them, we cannot but admit that the narrative given in the publication before us affords no little support to the opinion of the committee that the epidemic of 1855 was the result of the infectious atmosphere brought to Norfolk in the steamer Ben Franklin, and that it was by the addition of this atmosphere to the malarious atmosphere of the city that the latter became a cause of yellow fever in those exposed to its influence. That, however, the atmosphere, of certain portions at least, of Norfolk was in a condition ready to assume that peculiar morbific condition generative of the disease must be admitted in order to sustain the position of the committee, that the organic poisonous germs introduced into Norfolk by the infected steamer required, as a suitable nidus for their reproduction, an unwholesome atmosphere.

It is true that occasionally the committee in their report present statements as facts, rather because such are essential to the establishment of the views of the committee, than as clearly proved. A close investigation of the entire chain of evidence exhibited in this report in support of the foreign origin of the yellow fever in Norfolk during the summer of 1855 will, we think, discover more than one link entirely wanting or essentially defective.

We lay before our readers the general conclusions of the committee in their own language. In these we have a recapitulation of the leading facts in regard to the origin of the epidemic, with their bearing, in the estimate of the committee.

"1. The climate and situation of Norfolk cannot be very favourable to the development of yellow fever, inasmuch as during a space of fifty years—viz., from 1805 to 1855—the disease appeared among us as an epidemic but three times; viz., in 1821, 1826, and 1852.

"2. The year 1855, previously to the outbreak of the fever, was not remarkable either for heat or moisture. The temperature of the winter and spring, except April, was below the average of thirty years. That of June was about the average, but was exceeded in heat by fifteen Junes out of thirty. 'The heavy rains of April and May, followed by a long drought,' suggested in the Portsmouth report as the cause of the fever, did not exist. On the contrary, April and May were unusually dry, and June very wet.

"3. The city, if not in as good sanitary condition as could have been desired, was probably in as good order as most towns of the same size, and certainly as clean as it had been for the last twenty-nine years, during which we had the fever but once. There had been no recent filling up, and no excavation for gas- or water-pipes, &c. During the progress of the epidemic, the cleanest, dryest, and best-ventilated places enjoyed no greater exemption than the dirtiest

and most crowded, which would not have been the case if the disease were owing to the decomposition of animal and vegetable matter.

"4. During the first six months of the year the health of the town was very satisfactory. It was not merely as good as in preceding years, but, we think, better. In June and July there was much less dysentery, the most common disease of the season, than we ordinarily meet with.

"5. In all previous epidemics the disease began in Norfolk, and only extended, after the lapse of many weeks, to Portsmouth and Gosport, where it prevailed to a much more limited extent than in Norfolk. This circumstance was probably owing to the fact that all vessels from the West Indies come to Norfolk, and none to Portsmouth, which has had little or no commerce.

"6. In two of the three epidemics which have occurred in the last half century—viz., those of 1821 and 1852—the disease has been traced with great probability, if not certainty, to vessels arriving from West India ports with fever on board. That of 1826, of which, unfortunately, we have no recorded history, was attributed by popular opinion to a similar source.

"7. The fever of 1855 began in Gosport, the first cases being on the 30th of June, in a house adjoining Page and Allen's ship-yard, in a locality where there are no unusual elements of disease. The surface of the yard, it is true, was covered with chips, as is the case with all ship-yards; but, after a careful and minute personal examination of the premises, we have been unable to discover any obvious or probable cause of disease. The dock which has been pointed out as the '*fons et origo mali*,' the very focus from which the poison emanated, though partially filled with mud and logs of timber, is covered by salt-water at high tide, and, in our opinion, is altogether inadequate to the production of disease of any kind. The marshes around Gosport, and which are not in the immediate vicinity of Page and Allen's yard, are in much the same condition as they have always been—very unsightly when the tide is out, and giving rise, when partially obstructed, to intermittent and remittent fevers; but as they have never caused yellow fever before, in the last fifty or one hundred years, we are at a loss to understand how they could suddenly have acquired this new potency in 1855.

"8. On the 19th of June, eleven days before the first cases of fever were seen in Gosport, it is worthy of note, that this was about the time that has frequently been observed to intervene between the arrival of an infected vessel and the first cases of fever ashore—the steamer Ben Franklin from St. Thomas, came to Gosport, and was laid alongside the wharf at Page and Allen's yard, at the distance of not more than one hundred yards from the house in which the first cases occurred. This ship had lain a long time in the harbour of St. Thomas, where the fever was very prevalent. She had probably lost several of her crew while there, and had certainly lost two men by fever on the voyage to this port. When, after remaining thirteen days in quarantine, she came up to Gosport, there was a severe and fatal case of fever concealed on board of her. While she lay at the yard, three other cases occurred among those employed on board of her; viz., one on the 30th of June, another on the same day, or 3d of July, and a third on the 5th of July. Cases continued to break out on board of her after she was sent back to quarantine, not only among the crew, but in two negroes who spent a night on board of her—thus showing that the vessel was infected.

"9. The fever began in Norfolk in Barry's Row, on the 16th of July, just 17 days after its commencement in Gosport, 28 days after the Ben Franklin arrived at Page and Allen's yard, and 40 days after her arrival at quarantine. Barry's Row is situated northeast by north from Page and Allen's yard, and is 1.07 mile distant from it, with only water intervening. From the time of the arrival of the Ben Franklin at Gosport to the outbreak of the fever in Barry's Row, the wind prevailed almost constantly from the southwest. We have no evidence that the disease was conveyed to Norfolk in the baggage of fugitives from Gosport, as was currently reported at the time; but we incline to the opinion that the material cause of the disease was transported by the wind directly from Gosport or the Ben Franklin, and found a suitable nidus for its further development in the low, dirty habitations of Barry's Row. Nei-

ther was the distance—1.07 mile—too great, nor the time—28 days—too short for this occurrence. In this opinion we have been strengthened by the fact, that the next year, 1856, the yellow fever was thus communicated from vessels at quarantine to the Long Island shore, in the neighbourhood of Fort Hamilton, N. Y., where, when once planted, it continued to spread until frost.

"10. The disease did not break out simultaneously in all parts of the city, as it would have done, if produced by a combination of terrene and meteorological causes, nor was it scattered about as if by accident, as is the case with diseases propagated by contagion; but, starting from a central point, it advanced steadily and regularly in all directions, until arrested by frost—there being an interval of two months between its commencement in Barry's Row, and its appearance on the Princess Anne road, a distance of 1½ mile, and more than three months from the first case in Gosport to the occurrence of the disease in the extreme limits of Norfolk.

"11. The yellow fever north of the Gulf of Mexico is well known to be a disease of seaport towns only—never appearing in inland towns, unless previously existing in neighbouring seaports. When it occurs in seaports, it very generally begins on the wharves in the neighbourhood of shipping. We know of no causes, either local or meteorological, which are peculiar to seaport towns and the commercial parts of those seaports, and which are not equally found in inland towns, except the presence of ships from foreign ports. Accordingly, the common sense of the people, regardless of professional dogmas and theories, has always ascribed the origin of the disease to importation in ships.

"12. The fever in 1855 did not appear, in any case that came under our observation, to be communicated by contagion. Indeed, the greater number of instances in which persons from Norfolk died with yellow fever in other places, without communicating the disease, proves, as far as negative testimony can prove anything, that the disease is, decidedly, not contagious.

"*Finally.* After a careful consideration of the foregoing facts, we are unanimously of the opinion that the yellow fever of 1855 was introduced by the steamer Ben Franklin from St. Thomas, and that there is no reason to suppose that we should have been visited by the epidemic but for the arrival in our harbour of this or some other vessel with the fever on board.

"In the present state of science, it is not possible to determine what is the material cause of yellow fever, inasmuch as it is not tangible, and cannot be subjected to examination and analysis. It is only by a careful process of induction from all the known facts in relation to the disease, that we can hope to arrive at a correct theory of its origin and propagation. The hypothesis, which best explains most of the known phenomena of the disease, is that its material cause is some organic matter endued with the property of rapid reproduction, either in a soil or atmosphere congenial to it, but not capable of being reproduced in the human body. That this matter, whether of animalcular or vegetable character, is a production of tropical regions, and is only spread in temperate climates, when introduced into them by ships.

"As a necessary consequence of our opinions, we would advise councils that, while they should pay the strictest attention to sanitary regulations, such as cleaning, draining, &c., with a view to diminish, if possible, the spread and malignancy of the disease, in case it should unfortunately be introduced, yet our chief reliance for escape from yellow fever for the future must be in a rigid quarantine, at a safe distance, of all vessels arriving from suspected ports."

The report, with the foregoing conclusions as those deducible from the facts detailed in it, is signed by five members of the committee. The member whose name is not appended to it, dissents from the proposition contained in the 9th of the general conclusions asserted by his colleagues, "that the yellow fever poison was wafted by the wind from Gosport, and found a suitable nidus for its further development in the low and dirty habitations of Barry's Row, Norfolk." He "thinks it more likely that either the ferry steamer, plying twice daily between the original seat of disease at Gosport, and the vicinity of Barry's Row, or the workmen on board of her, acted as the communicating agent of disease."

In reference to the question of contagion, his observation, he remarks, "favoured an affirmative opinion." He thinks that the cause of yellow fever is some minute material germ, capable of reproducing itself when given off from the human body suffering under the disease.

If, he remarks, he should hazard any opinion in addition to that given in the report, it would be "*to remove all persons sick of yellow fever as far as possible from a crowded city population, and especially from that of the labouring Irish, who now abound* in Norfolk."

D. F. C.

ART. XXIII.—*Principles of Medicine. An Elementary View of the Causes, Nature, Treatment, Diagnosis and Prognosis of Disease, with Brief Remarks on Hygienics or the Preservation of Health.* By CHARLES J. B. WILLIAMS, M. D., F. R. S. A new American from the third and revised London edition. Philadelphia: Blanchard & Lea, 1857. 8vo. pp. 486.

THE views of the author in preparing this work are set forth in the first paragraph of the preface to the present edition, as follows: "In this work the attempt is made to place the practice of medicine on a footing somewhat corresponding with that of physiology, chemistry, and other sciences which equally depend on the accurate observation and rational arrangement of facts. If our knowledge of the healthy body, and of its parts and functions be exact, it ought to be our best guide in the study of the same parts and functions in a state of disease. Yet the belief has long prevailed, and even still has its avowed advocates, of regarding the phenomena of disease as a new and separate order of things, the character and laws of which are to be investigated by themselves, and without reference to the standard of health from which they deviate; as if diseases were independent entities, and as if the body under their influence ceased to possess the same structures and functions which it has in health. Happily, however, such irrational dogmas do not now find much favour with the members of our profession, who, in proportion as they become more enlightened by sound physiology, recognize in it when continued with careful clinical and pathological observation, the best guide to the understanding and treatment of disease."

We quote these views as consonant with sound medical philosophy and the true basis of the principles of medicine. The tendency of the mind to oscillate between extremes has been exemplified by the currents of medical opinion during the last twenty-five years. At the commencement of this period the practice of medicine was wholly subordinate to medical doctrines. The practical notions of physicians were determined by their speculative opinions, leading to directly opposite methods of treating diseases according as they adopted the doctrine of Brown on the one hand, or that of Broussais on the other hand. Theoretical medicine was, in fact, *a theory* in the ordinary acceptation of this term, and practical medicine was, in other words, a system of practice constructed in strict conformity with theoretical principles.

Clinical observation and numerical analysis marked a new epoch. Medical opinions and practice were to be emancipated from the tyranny of theoretical dogmas. These methods of investigation, heretofore too much neglected, already productive of great results, and promising for the future incalculable benefits to medical science, have yet, in some minds, been invested with an importance too exclusive. The extreme in this direction is exemplified in the notion that the principles of pathology are deducible only from observations at the bedside, and that empirical knowledge is the sole basis of therapeutics.

Sound philosophy and common sense dictate a middle course between these extremes. To develop the laws of disease and the effects of remedies by accurate observation and statistical researches, are objects which cannot be overrated except by regarding them as the only means of arriving at truth. On the other hand, the direct pathological bearing of progressive increase of our knowledge of anatomy and physiology, must not lead the medical philosopher to disparage the labour of the clinical observer and analyst. True it is

that the phenomena of disease are but the phenomena of life presented under an abnormal point of view. The distinction between physiology and pathology is arbitrary; in reality they constitute one science, and just in proportion as we become more correctly and intimately acquainted with the structure and operations of the living organism in health, are we qualified to understand more fully the alterations which this organism presents in disease.

These remarks may seem trite to some of our readers. They have been suggested by the quotation above given, and do not seem to us out of place in view of the fact that the two extremes of medical opinions just indicated, have each its representatives in the ranks of the profession.

Dr. Williams' work on the principles of medicine has been for several years well known to medical readers on this side of the Atlantic. The call for a republication of the third edition is evidence of the favour with which it has been regarded. Much the larger portion of the volume (327 pages) is occupied with the consideration of the primary and proximate elements of disease and structural affections. The remainder (159 pages) is devoted to etiology, semeiology, diagnosis, prognosis, and hygienics. The author's plan is to exhibit a concise view of the deviations from the normal composition, structural arrangement and functions of the organism so far as these are at present known, and to trace the rational applications of this knowledge to the practice of medicine.

To any who appreciate the activity and progress characterizing the study of the organism in health and disease, it must be sufficiently apparent, that the rational principles of medicine at the present moment cannot be expected to be altogether permanent. In fact, the principles of medicine now, or at any given period, must be, to a greater or less extent, provisional. Some practical truths will remain unchanged, but the greater number of principles which appear to be, and indeed are most consistent with existing knowledge, will, in all probability, undergo important modifications, if not displaced by new views which, in their turn, may be compelled to give way to others more consonant with the progressive developments of science. If the instability of pathology be made an occasion for reproach, who does not see that medicine would be far less entitled to our respect had a misguided conservatism held tenaciously to all the doctrines in vogue but a few years ago! Who does not see in this very instability encouragement to look for future developments which may vastly enhance the character of medicine as a science and its usefulness as an art!

The plan of Dr. Williams' work we regard as well devised, and in the main, well executed. Although the first edition was published but a few years ago, the advances made in the several departments of biology render necessary important modifications of some of the views enunciated in that edition, as well as newly added views. It is intrinsically difficult to adapt successive editions of a text-book in any science to the changes incident to the progress of the science if the latter be actively progressing. This difficulty is apparent in the present instance. Moreover, in bringing a new edition up to the existing state of the science, an author's partiality for his work inclines him to make the requisite alterations appear as small as possible. His egotism is wounded by striking inconsistencies on comparing late and previous editions. The justness of this remark is evidenced in this instance. Considerable ingenuity is shown in attempts to trace an identity in the observations contained in the first edition and in recent scientific developments. Were we to write a review of the work (which at this time is not called for), we should find scope for criticism in the overweening desire which the author manifests to maintain consistency of opinion; in the representations of opinions or hypothesis in the light of well established facts; in the looseness of certain statements, and a disposition to attempt to explain everything instead of fairly confessing ignorance when our present knowledge is defective. In view of these blemishes, a newly written work on the principles of medicine might be made more acceptable, and seem to us to be in a measure needed. Until we have a work embodying more freshly the facts and deductions pertaining to medical pathology at the present stage of its progress, the "Principles of Medicine" by Dr. Williams will maintain the position of an useful elementary treatise.

A. F.

ART. XXIV.—*Report of the Recent Yellow Fever Epidemic of British Guiana.* By DANIEL BLAIR, M. D., Surgeon-General of British Guiana. 8vo. pp. 91: London, 1856.

THE Report of Dr. Blair is one of considerable interest. Besides presenting a summary of the history of yellow fever as it prevailed in British Guiana, in the years 1852 and 1853, it indicates several important points in reference to the symptomatology and morbid anatomy of the disease that have not been prominently noticed by previous observers.

From the beginning of 1845, the health of the seamen in the harbour of Georgetown, Demerara, and of the inhabitants, generally, of the entire colony of British Guiana, was, we are informed, good. During four months of 1850, from June to September inclusive, mumps became epidemic and epizootic, being very fatal to cattle. From the latter part of July to the latter part of August, of the ensuing year, a malignant influenza swept over the country and destroyed many of the feeble and dissipated among the Coolie and Portuguese immigrant population, but was scarcely felt by the seamen. In fact, until the end of 1851, the harbour of Georgetown might have ranked, we are assured, among the healthiest in the world; and no disease existed in the colony of which the newly arrived European or North American need have had the slightest apprehension.

The years 1851 and 1852 were remarkable for the favourableness of the seasons, indicated by the extraordinary average yield of the sugar crops. From 1849, a great change took place in the distribution of rain over the colony. That year was the acme of the rainy years. Previously, and up to 1851, the rain and dry weather appear, in the meteorological charts, gathered up in large masses, but since then there has been a less quantity, and greater dispersion. During 1851, the rain was so equally distributed over all the months, that no great washing or drying of the country took place.

"The meteorological characteristic of the weather preceding and accompanying the advent of the yellow fever, therefore," remarks Dr. B., "was *the absence of any decided dry, or any decided rainy season.* It was favourable to vegetation and agreeable to the feelings; and the minimum temperature of six years occurred in the month of January, 1852 (13th), when the thermometer fell as low as 67.7°. The coincidence of the invasion of yellow fever with the most cool and agreeable time of the year, corresponded in this respect with the epidemic that preceded ours along the windward coast of South America; that of Cayenne having commenced about the end of November, 1850, and that of Surinam about the end of January, 1851.

"Although our former epidemic had every appearance of local origin only, that from which the colony now suffers would seem to be the result of some general exciting cause acting consecutively along the southeastern seaboard of America, which, beginning at the Brazils, passed on to French, then Dutch, then British Guiana—thence to the West India Islands, New Orleans, and, finally, Bermuda. Had the winter not interfered, probably Philadelphia and New York would have been reached. Although, if its diffusion was due to the agency of the trade-winds solely, whose course it followed, the latitude of Bermuda should have been its terminus. The hypothesis of a great epidemic wave, rising in the east, and flowing on westerly, only apparently suffers from a minute inquiry into its course—for, although Demerara was invaded at the end of 1851, while Berbice, which is easterly, or to windward, did not suffer seriously till the end of July, 1852, still, in New Amsterdam, the port and capital of the latter country, two fatal cases occurred as early as February, 1852, and one in May following; and it is to be considered, that this town (unlike Georgetown) is situated several miles above the mouth of its river, and may have been caught, so to speak, in the eddy of that great epidemic wave, which so peculiarly affects the sea margins.

"Although the present epidemic has been apparently more intense and dif-

fusive than its predecessor, and its origin seems referable to a foreign source, still it affects special localities as before; and the tenements which suffered most on a former occasion, have been again those of its severest visitations."

"The march of the epidemic, its dates and lines of diffusion, would indicate the influence of atmospheric currents on its progress. Outside the boundaries of epidemic influence there was safety. The danger seemed in some measure proportioned to the nearness of approach to the centre of infection; and several striking instances have occurred, of parties descending, on a visit, from the uplands in the interior, and the unaffected regions of the coast, falling victims to the infection of the town. Within its circumscribed range, the epidemic manifested local predilections, and though some places seemed permanently affected, the lines of infection occasionally shifted, as in the former epidemic, and infected and unaffected localities were temporarily in juxtaposition.

"Lulls and exacerbations in the general violence and intensity of the epidemic were frequently observed in its course. The first of these lulls occurred in the last half of the month of March, and the first exacerbation in June. By the end of August, another lull, but of short duration. In February and March, 1853, the epidemic power was intense. It moderated again till June, when it was renewed with great virulence. These lulls in the epidemic were as illusive as the lull of symptoms in the fatal progress of the disease; and it was often my painful duty to discourage the hopes that were so eagerly entertained by the authorities and the public, of the entire and speedy disappearance of the epidemic, and to resist, with apparent pertinacity, the repeated proposals for the return of the white troops to the military service of the colony.

"Although the epidemic sprung up at a delightful season of the year, when the general health was excellent, and, perhaps, irrespective of weather, yet, in its course it seemed materially influenced by meteorological conditions; and sometimes even diurnal variations were observable in the condition of the whole of the patients in the hospital, which could only be referable to atmospheric causes. A cool, dry, brisk air seemed to have a mitigating effect, while a hot, sultry, close, moist air increased the number of admissions, and aggravated the type of the disease, particularly on its immediately following the other meteorological state."

Dr. Blair notices the occasional blending of yellow with intermitting fever, and its modification in some cases by influenza. In June, 1853, smallpox became very prevalent, and was suspected in some cases to be of spontaneous origin. Mixed cases of smallpox and yellow fever occasionally occurred, the former disease predominating; if, after the stage of desquamation, smallpox patients were attacked with yellow fever, the latter then had its own course, unmolested. The coexistence of pneumonia and pleuritis with yellow fever, sometimes the one, and sometimes the other being primary, was of frequent occurrence, particularly among the Portuguese immigrants.

It is stated by Dr. B. that, in the course of the epidemic, several long-standing cases of chronic disease, to the consternation and surprise of the bystanders, terminated suddenly and fatally by black vomit without any precursory fever.

Not only was the epidemic, as a whole, subject to modifications and fluctuations, but the early individual symptoms of the disease also.

"Sometimes," remarks Dr. B., "the full complement of standard symptoms were present, sometimes they were imperfect or deficient, and sometimes displaced. At one time the diagnostic symptom was the supra-orbital headache. This, in the epidemics of Cayenne and Surinam, seems to have been the constant characteristic, accompanied, generally, by lumbar pain. At other times, the tongue symptoms alone were diagnostic. Sometimes their equivalent was observed in the fauces and uvula. In the Surinam and Cayenne epidemics our tongue symptoms do not seem to have been at all recognized. These variations and shiftings of the symptoms were not irregular or promiscuous, but *periodical;* and they continued steadily for several weeks together." "Intense surface heat, early albumen in urine, and early black vomit, were the character of the later symptoms; and smoky, pale urine, with perfect blood-corpuscles, took

the place of the straw-coloured or bilious urine, with its sediment of tube-casts and epithelial matter. Notwithstanding this variation of symptoms, they were never so defective as to prevent the formation of a correct estimate of the nature of the disease with which the practitioner had to deal. The variation of symptoms had sometimes a relation to the mode of accession of the disease. In the diarrhœal or choleroid cases, the tongue and head symptoms were seldom so early or developed."

It will be impossible for us to follow Dr. B. in his most interesting account of the several symptoms of the disease.

The supra-orbital pain, and punctated tongue, Dr. B. considers as most valuable premonitory symptoms, the first, especially, giving notice of an impending attack several days before it occurs. These symptoms, also, indicated the epidemic taint in some cases of intermittents, which resisted the ordinary treatment of such cases.

The frontal pain seems normally to belong only to the formative and febrile stages of yellow fever, and subsides spontaneously in the middle and late stages. It is sometimes described as in the orbits, more rarely in the upper part of the forehead; occasionally as extending to the occiput. It is usually attended by an increase of temperature over the forehead.

Dr. B. describes a *specific capillary irritation* that shows itself in the flush of the face, and which he pronounces as characteristic of yellow fever as the hectic of phthisis, or the fuliginous complexion of typhus.

"This suffusion," he remarks, "generally occupies a zone over the eyes, and about an inch above and below them. The eyes are injected, like those of a person *just awake*, but generally without any lachrymation or photophobia, although the injection may be as intense as in ophthalmia. Sometimes the irritation extends to the palpebræ, to one or both, and sometimes only one eye is affected, but that so violently, as if the patient had been *stung*, or received a blow on the eyes." "The nares also may be found injected, with a coarse vascularity; the lips may be crimson or vermilion coloured; the tongue scarlet at tip and edges. If the fauces be examined, the roof of the hard palate will be sometimes found covered with a coarse network of capillaries, which extends to the uvula. If the practitioner is already satisfied as to the nature of the case, this examination of the fauces should be omitted, on account of the *vomituritis* generally induced by pressing down the tongue preliminary to the examination."

Dr. B. also remarks, that, upon a careful examination of the chest, a subcutaneous rash may sometimes be observed, which occasionally extends to the arms and abdomen. The face, chest, arms, and legs have likewise, in some cases, a slight purplish appearance after the second or third day, and sometimes the colour of a boiled lobster.

"This appearance varies much in degree, but may be detected by pressing the hand flat on the chest, when the fingers will for a short time be delineated in white with purple outline. This symptom occurs chiefly in the 'smouldering' form of the disease, and is often so deep as to conceal the jaundiced appearance of the skin."

The appearance of this *languid capillary circulation*, as it is called in the hospital case-books, is looked upon, says Dr. B., with favour. It would seem to indicate, he adds, that the congestions were directing themselves towards the periphery of the body, thus relieving the vital internal organs from a part of their load. In these cases the skin is generally cool and moist, and sudamina occasionally appear.

The appearance of the tongue in yellow fever, and the variations presented by the organ in different cases, and in the different stages of the same case, are minutely detailed by Dr. B.; in these details, however, interesting as they are, we cannot follow him. The same remarks will apply also to his description of the condition of the urine, and the changes it undergoes in the course of the disease. He describes it as always acid in the first stage, and continuing so generally until convalescence, when it becomes alkaline, or until it becomes heavily charged with bile. He confirms the accuracy of Dr. Collings' observations in regard to albuminosity of urine as a characteristic of yellow fever. The

albumen generally appears on the second or third day; it has been found as early as the first day of illness, while, in a few cases, it did not appear till the day of death, and after black vomit had set in. It was seldom seen in aborted cases. In a few instances it appeared in these during convalescence. In three cases the albuminosity was *intermittent* for one or two days.

"Albumen appeared in every fatal case of normal duration. It sometimes ceased in convalescence suddenly, always before the yellow suffusion of skin and eye, or bile in the urine, disappeared, except in a single case, where Bright's disease seemed to be a sequel of yellow fever; the patient, after remaining in hospital upwards of two months, left with his urine still albuminous. Between the eleventh and twentieth day of gravior cases, it generally disappeared, and its disappearance formed the criterion for the discharge of the patient from the hospital. The colour of the precipitated albumen was never *white*, as it is in our cases of Bright's disease."

"The *turbidity* of the urine was not necessarily connected with its albuminosity. The urine may be deeply tinted with bile, and highly albuminous, and yet clear. The turbidity of the urine was caused by the presence of mucous epithelial matter, coagulated albumen, and casts of the urinary tubuli. It is probable that the free acid of the urine has a coagulating power, and sometimes communicates turbidity. The presence of mucus will have a similar effect, but the turbidity then is not general, but occupies a lower stratum of the fluid, and is light and floating there, while the supernatant liquid is clear. There is nothing distinctive or of importance in this mucous condition when the urinary secretion is copious. Perfect epithelial scales are rarely found in the sediment, but broken up epithelial matter is abundant." "Although albuminosity is almost always the antecedent to the presence of tube-casts, a case occurred in which these were found in non-albuminous urine."

The albuminosity of the urine furnishes, Dr. B. observes, one of the most obvious manifestations of yellow fever entering its second stage, and its extension to the great solid viscera.

"Cases have died even when the urine was full and free; but life is prolonged thereby; and no guarantee of safety in one epidemic was so sure as an unobstructed action of the kidneys; and no sign, not even black vomit, so dooming as a suppression of urine. Hope then was gone. No matter how desperate the condition otherwise, if there was copious transparent urine, though ever so coagulable, and black as ink from bile, the struggle was hopefully maintained. For it was felt that the system was still competent to the elimination or decomposition of the yellow fever poison. But *suppression* after the abundant appearance, or curdy aspect of albumen and tube-casts, rendered despair reasonable. The scanty oily-looking urine was generally present in cases that might be abandoned. The tube-casts had disappeared—for the capability of *washing out these plugs of the urinary tubules* no longer existed: they are irrecoverably choked; and the bulk of the scanty secretion seemed to be derived, not from the kidney, but the bladder itself."

A very instructive chapter is given by Dr. B. on the ejections from the stomach in yellow fever. In the early period of the attack the matters vomited are alkaline. When the second stage of the disease sets in, on the second, third, fourth, or as late as the fifth day of the disease, they become acid, and continue so to the close of the disease; the acidity being most decided during the production of black vomit. The acid discharge may, at first, consist of a quantity of clear, pale, almost limpid or slightly opalescent fluid. With this ejection the disease may terminate, or go on to a protracted period, without change in the matter vomited, usually, however, it is the precursor of the black vomit, the acid ejections becoming mixed to a greater or less extent, with snuff-like specks, before it merges into well-defined black vomit. "*Normal black* vomit," is described by Dr. B. "as having a laminar or granular sediment, of a deeper or paler shade of brown, sometimes verging on jet black, with a clearly defined supernatant serum of low specific gravity, and without mucosity, partaking of the colour of the sediment, but sometimes nearly limpid when the sediment is black, as if all the colouring matter had subsided. Many de-

viations from this standard occur from the presence of ingesta, hemorrhage, and excessive secretion of mucus."

"The sediment of black vomit seemed to consist of coagulated albumen and the débris of blood-cells. In no case in which the black vomit was normal to the eye, was a single perfect corpuscle observed. When pressed through a paper filter the colour is rendered considerably paler. The sediment of black vomit seemed more highly acid than the supernatant liquid—it makes a stronger impression on the test-paper. The sediment acts as a ferment on liquids containing sugar."

"The presence of ammonia in black vomit," according to Dr. B., "is universal, that is, it has always been found when looked for, and may be considered one of the tests of black vomit."

"During the former epidemic," Dr. B. informs us, "it was noticed in cases of black vomit, that, when it *preceded* the yellow suffusion, the prospects of life were improved. The relations of this fact were not then understood. Black vomit is significant of imminent danger, from the circumstance that it is the *dernier ressort* of nature to relieve that contamination of the circulation which has been produced chiefly by impairment of the function of the kidneys, and the retention thereby within the system, of the worn-out nitrogenous elements of the body and their poisonous metamorphoses. Now, if black vomit appear early in the disease, before its march has extended to the great internal viscera; before the bile function has been disturbed, or the urine rendered albuminous, it ceases to be the significant symptom which has obtained so much ill-omened celebrity. It is then the sign of a local, instead of a constitutional affection."

In the ensuing chapter we are presented with the observations of Dr. B., in reference to the condition of the blood in yellow fever, as relates to the epidemic of which he treats. From these observations it appears that during the first stage of the disease, in no instance could there be detected any really abnormal condition of colour, corpuscles, serum, or crassamentum, except sometimes the presence of bile. Changes of the blood were met with only in the last stages, and after death. Cases, nevertheless, terminated fatally after normal black vomit and hemorrhages, in which no unhealthy appearance of blood after death could be observed, except as to the bile tinge. The appearance of yellow fibrinous coagula in the heart was frequent after the worst cases, and what may be termed the texture of the blood often remained good. Blood passed off by stool, though of good crassitude and colour to the naked eye, is always found under the microscope with all its corpuscles ruptured. Which alteration Dr. B. considers to be, in great measure, due to chemical changes which occur after its extravasation.

During life, however, the blood is sometimes found altered. This alteration consisted chiefly in a change in the shape and condition of the corpuscles, and a deterioration of the fibrin. The fibrin is sometimes greatly diminished in quantity, at others, it seems to have lost, in a great measure, its power of fibrillation. The albuminous element appeared, by the rough test of its becoming solid by heat to coagulation point, to be sufficient.

"The healthy condition of the blood in yellow fever," observes Dr. B., "seems associated with free action of the kidneys, or copious black vomit, and alkaline exhalations of the breath. And the deterioration of the fibrin has an obvious relation to the amount of *free* ammonia remaining in the circulation. The changes in the *shape* of the corpuscles are probably due to alterations in the density and saline constituents of the serum. The blood of the cadaver in this epidemic, was in the vast majority of cases more or less ammoniacal."

In chapter 8, we are furnished with the observations of Dr. B. on the ammoniacal breath of yellow fever patients. From these observations it became apparent that the urea of the suppressed urine is eliminated from the system as a volatile salt, by its metamorphosis into a carbonate of ammonia, which, as such, is frequently found in the breath, occasionally in the black vomit and hæmatemesis, almost always in the stool; in two cases in the urine, always in normal black vomit in combination with an acid, and, indeed, apparently pervading all the tissues of the body.

In the chapter on the sequelæ of yellow fever, Dr. B. states that relapses

were of frequent occurrence, occasioned most likely, in great measure, by a return of the patient to the focus of infection after discharge from the hospital. Those relapses, we are told, were almost exclusively among the aborted cases. They frequently recurred, and were aborted several times. The primary attack was generally, but not always, without albuminosity of urine, and frequently the relapse also; generally, however, in the relapse albuminous urine was expected, even if the disease was again aborted, during convalescence. Only two relapses occurred after the disease had run on to black vomit, both of which were readily aborted, although in one the relapse proceeded to albuminous urine. The tendency to relapse or second attack was generally within the first month after the primary attack.

Dr. B. describes four distinct varieties in the mode of death in uncomplicated yellow fever; and these are sometimes blended: namely, syncope, uræmia, apoplexy, and asphyxia. Death from syncope may arise from excessive discharges of black vomit, or from hemorrhage, as excessive epistaxis, bleeding from the mouth and gums, or from the black vomit and hemorrhage combined. If before death the urine be suppressed, and the black vomit is not copious or has ceased, the circulation becomes contaminated, and when this condition acts upon the brain in its mildest form, the effect is not unlike alcoholic inebriation. In one case the patient, on the night of his death, sat up in bed, drank beverages, and joked with the shipmasters around him; in another, the patient, within a few hours of his death, was found sitting up in his chair, and regaling himself with his tobacco pipe. If all the excretions and secretions be locked up, as occasionally happens, the symptoms of uræmic poisoning become more violent, the sensorium painfully affected, irritability of temper, screams, and wild ravings ensue, followed by convulsions, coma, and death.

Death by apoplexy, caused by congestion and extravasation of blood on the brain, was observed in several cases. In one case death from asphyxia was the result of laryngeal suffocation, and in two, of pulmonary apoplexy.

We must pass by without notice the chapter on the pathological anatomy of yellow fever, though replete with interest, and can only give the following concluding paragraphs of the chapter on its diagnosis and prognosis.

"Yellow fever, although it may be engrafted on an intermittent, when once formed, has no intermissions. It is a fever of one paroxysm, without the crisis of perspiration, and when it is over, health is restored, or the disease goes on inducing its ultimate changes without febrile action. The time of seizure is different with yellow fever from that of our permanently endemic fevers. It generally comes on in the night-half of the twenty-four hours; while with us, all our miasmatic fevers, whether quotidian, double quotidian, or tertian, in the immense majority of instances, occurred at mid-day. And, if we follow intermittent into its sequelæ, we find no resemblance between the two diseases. There is not the quick restoration of health usual in yellow fever, nor the bloody furuncles of unhealthy convalescence, but, instead, enlarged spleen, anæmia, dropsy, and colliquative dysentery."

"The number of the characteristic symptoms present, and the degree in which they are manifested, furnish criteria of the severity of the case, and the ratio of danger. A slow pulse and moderate temperature of the body, and quiet stomach, are always favourable indications. But the more fiery crimson the tip and edge of the tongue, the more irritable the stomach, the severer the headache, the worse the prognosis of the first stage, and *vice versâ*. Slight or moderate epistaxis is a sign of little prognostic value in any stage; but streaks of blood in the early vomit, indicates much danger; while the same, during the stage of black vomit, or after acid elimination has set in, is favourable, if the corpuscles are found entire. In the second stage, the earlier or more complete the suppression of urine, and the more copious the ejection of black vomit, the more imminent the danger. But if the urinary secretion continue, and the black vomit be scanty from the first, or is afterwards suppressed, the patient may yet survive. Urine simply albuminous is a less serious sign than when it also contains tube casts; but if these are thin and few in number, they do not add much to the gravity of the indication. Free, copious urine, no

matter how dark or bilious, is the most favourable of any single sign. If the urine be scanty, and it be loaded with tube casts, entangled in epithelial and fibrinous matter, forming a light buff coloured sediment, it indicates a complex lesion of the secreting structure of the kidney. It is the urine symptom in its maximum of severity, and is as fatal as if the suppression had already occurred. Blood corpuscles in the urine were not looked on with apprehension. A faltering of the articulation is a bad prognostic, and a difficulty of protruding the tongue enhances it." "The danger of the case is enhanced by its inflammatory complications, and by hypertrophy of the heart. The recency of residence in a temperate climate, the *race* or complexion of the individual; the fact of his previously having suffered from a gravior attack, or an aborted one, will enter into an estimate of his chances of recovery."

In the ensuing chapter some of the points connected with the etiology of yellow fever are discussed.

From the observation of the epidemic of 1852-53, it would appear, that, although a certain high average temperature is required for the generation and continued existence of the efficient cause of yellow fever, it has not its genesis from any known combination of meteorological elements, and may appear at a time when these are highly favourable to general health and comfort. It would appear further, "that the laws of its diffusion differ from those of gases; that it is impelled by atmospheric currents, but seems to possess some power of locomotion," as indicated by its shifting lines of infection and gyratory movements; "that the development of its power was gradual, from its feeble and diluted manifestation at the end of October, till its perfectedness at the end of December, and its maximum of intensity a month afterwards; that during the course of its progress it showed marked variations of epidemic power; that in constitutions apparently the same, the system was affected in various degrees, as if the poison acted in proportion to its quantity, and as a poison and not a ferment; that its first impression on the system seemed in many cases local and circumscribed, although attended with the usual constitutional disturbance; that it can actively occupy the body simultaneously with other affections, and may be either subordinate or paramount in the issue; that though the extensive application or saturation of the system by the efficient cause eventuates in a spontaneous outbreak of the disease in the individual, there are circumstances which accelerate its action and augment its intensity, and others which retard or entirely obviate and render it inert."

Previously to considering these two set of circumstances, Dr. B. considers those facts which bear upon the question of contagion. Numerous observations are adduced in which the freest intercourse with the sick took place without the occurrence in a single instance of the unquestionable propagation of the disease; one or two cases, however, are referred to, which, it is admitted, may be open to suspicion, till all the circumstances in connection with them are explained.

"I have," Dr. B. observes, "stated thus all the facts that have come to my knowledge during the course of the epidemic, which favour the doctrine of the personal transmissibility of yellow fever. They were earnestly looked for among the countless opportunities for observation, and no others could be discovered. Those which were found have been honestly declared. In such a poverty of positive proof in the affirmative of the doctrine, it is no argument against those who disbelieve in the doctrine of contagion, to assert that their proofs amount to negative evidence only. The experience of the present epidemic has confirmed that of the past, and the idea of contagion, which was then unanimously relinquished, has not been revived. Neither do facts countenance the fanciful compromise which some have offered as a settlement of what is scarcely a question among those who in modern times have seen the disease with their own eyes, viz: that it is *the type of disease in which black vomit appears only which is contagious.* In Demerara we would as soon think of asserting that intermittent fever in some of its forms and types is contagious, as to predicate it of any of the manifestations of yellow fever."

Among the causes which accelerate the action and augment the intensity of the efficient cause of yellow fever, Dr. B. enumerates, heavy rains, with calms,

creating a damp, hot, steamy atmosphere, or the prevalence of land winds, which are cold, comparatively damp, and of low dynamic power. The return of a dry, cool, clear, elastic atmosphere, with sweeping trade-winds from the ocean, was always followed by mitigating effects. The *rationale*, Dr. B. remarks, is easy. The condition of the weather first referred to oppresses the cutaneous and pulmonary functions, and thereby lowers the tone of health and its power of resistance to the action of noxious agents, at the same time that the stagnation of the air is favourable for the accumulation of the atmospheric poison, whatever it may be.

The next predisposing and intensifying circumstance noticed is locality. In the last as in the former epidemic, the poisonous agent persisted in its predilection for low, damp, crowded places, and the neighbourhood of putrid exhalations, and woe to the unwary or reckless who lived or lingered in such places or exposed to such exhalations.

Fatigue and checked perspiration and long continued solar exposure also tended to precipitate the attack. The tolerance of the poison which those residents who had passed through the epidemic from its first feeble manifestations, had acquired, was seriously impaired by even a temporary removal from the colony, and a return to it within a few weeks. The depressing emotions of the mind were highly favourable to the action of the poison. Worry and vexation, crushing sorrow, panic, and even overwhelming joy, have each had its victims. Among the exciting causes of yellow fever, Dr. B. enumerates the presence in the body of other febrile and irritating affections. A paroxysm of intermittent fever, he remarks, would sometimes set the morbid train in motion, as also the primary and secondary fever of smallpox, or an attack of pneumonia or even bronchitis. The stench of bilge water seemed sometimes to be an exciting cause.

"One of the most favouring causes of the action of yellow fever poison was," according to Dr. B., "infancy. The constitution of the new born or young white creole was highly susceptible. He or she was truly in the category of new comers. Not only did the first cases in town occur in children, but they followed numerously and repeatedly. As these infants and children were not exposed to some of the physical and moral influences which favoured the attack in adults, their high susceptibility can be imputed to structural differences only.

"Many facts," says Dr. B., "came to my knowledge, which showed that *family predisposition* for yellow fever exists, and is evidenced under varieties of exposure. It was noticed in several cases that a scorbutic diathesis, or sponginess of gums in the individuals attacked, prognosticated the worst results. But the great predisposer—the pabulum on which the epidemic revelled—was the organization of the white who had recently arrived from an elevated or mountainous country beyond the tropics.

"On the other hand, in looking for the causes which operate in retarding, or mitigating, or entirely shielding from the action of the yellow fever poison in the infected localities, we find that cheerfulness of mind, active but not laborious occupation, regularity of habits, and avoidance of night air, sustain the tone of health and militate against the inroads of the prevailing disease. The appearance of the eruption of smallpox seems to supersede the yellow fever poison. The presence in the system of evacuant diseases, such as the advanced stages of phthisis when the tubercles have softened, and even gonorrhœa, seems to have a retarding power. Several instances in the hospital were observed of attacks supervening on the *heading up* of the discharging surfaces of burns, scalds, and wounds.

"Of all the protections, that of *complexion* was paramount. When the ships' crews were disabled by sickness (and that was in the majority of instances), their places were supplied by negro sailors and labourers. On board of many vessels, black labour alone was to be seen employed, yet among those labourers and stevedores a case of yellow fever was never seen." Of 7890 African (black) immigrants, none contracted yellow fever.

We have already so far exceeded our limits in the notice of Dr. B's. report

that we can afford room for a brief outline only of the chapter on treatment.

The primary object of treatment was, if possible, to abort the attack by the administration of a mixture of calomel and quinine. Twenty grains of the first, and twenty-four of the second, for an adult, given in some simple syrup or pap. This dose was repeated at intervals of four or six hours, to the extent of four doses, unless the attack was earlier arrested. If after three or four doses are given the disease is not cut short, little room is left for active interference on the part of the physician, although still much may be done in putting the patient in the best condition for sustaining the struggle, and keeping off intruding complications.

In preparing the irritable stomach for the reception of this abortive dose, creasote, we are told, had often an admirable effect.

The first dose of the calomel and quinine was followed by a suitable dose of a purgative mixture, composed of drachms ij of carbonate of magnesia with oz. ij of sulphate of magnesia, in oz. viij of peppermint-water.

The aborting dose of calomel and quinine should be used as early as possible in the attack. When a state of apyrexia is induced, it may be relinquished, the end is attained; but if the urine has become coagulable, or the epithelium of the tongue has begun to peel, it is of no use pushing it further, the time for its use is passed, and subsequent to this it will be a noxious irritant.

"Sometimes the disease is incompletely aborted, that is, although it does not proceed to the second stage, a certain amount of febrile action still continues after the resolvent has been pushed to a reasonable extent. It was the practice then to give half an ounce of camphor water and spirit of mindererus every three or four hours, till the skin became cool and soft. Should, however, the stage of acid elimination supervene, this medicine is stopped, and small doses of bicarbonate of soda and nitre (five to ten grs. of each) substituted."

When the mucous surfaces, as indicated by the tongue, were denuded of epithelium, the use of gum-water was decidedly beneficial. It lubricated, defended, and soothed the raw surfaces. The strength was generally three drachms of the purest powdered gum dissolved in six ounces of cold water; a tablespoonful being given every hour or two. For thirty-six or forty-eight hours of the most critical period of the disease, it is used without dissatisfaction on the part of the patient; after that it can be substituted by, or alternated with, arrowroot pap.

"When the heat of surface was ardent, the *wet sheet* or *blanket* was used for the reduction of temperature by evaporation, with frequently very good effect. But in the late stages of the disease, when the skin was cool or cold, the patient seemed to have an instinctive craving for its reapplication, and frequently asked to be put into it. There would appear to be two causes for this feeling. We find it to exist in cases in which black vomit has been copious, and the associated thirst distressing; also where there has been no black vomit of any consequence, and the breath is highly ammoniacal. In the former case the stomach ceases to be an *absorbing* viscus in anything like the proportion of its secretions and transudations. The skin is therefore employed in reducing the crasis of the blood by the absorption of water, as shipwrecked mariners are said to quench their thirst. But not only does the skin afford an inlet for the imbibition of diluting fluids, but the softening of the cuticle would seem to afford an additional outlet for the noxious elements of the circulation, and it is probable in this direction we must in future look for auxiliary means of relieving the blood of its poisonous metamorphosed and effete constituents, the onus of which is now thrown on such vital organs as the stomach and lungs.

"The *food* during the course of yellow fever should be of the blandest description; chicken tea, arrowroot, sago and barley water constituting the chief articles; and these should be taken when the stomach is at all irritable, in minute quantities at a time. This rule also applies to drinks of all kinds. The patient is greedy for a large draught of fluids, but by sucking them through a glass tube of small bore, or taking them by the tea or tablespoonful, they are much more likely to be retained. A cold infusion of oatmeal was found an

agreeable drink for the Scotch seamen, of which they did not seem to tire. A dislike of sweets was observed among the patients, and when lemonade was asked for, the usual quantity of sugar was objected to. Tea was found uniformly to disagree with the patients, and cause vomiting, particularly in the advanced stages. Dilute alcoholic drinks were given freely, and with good effect."

During the course of the disease, *auxiliary treatment* was required to meet contingent symptoms. Cupping, leeching, and blistering were found useful in relieving the primary head symptoms and irritability of stomach, when applied respectively to the nape of the neck or epigastrium. Tenderness over the liver seemed also benefited by these applications. Dr. B. has never seen any benefit resulting from their application over the kidneys, with the view of relieving that congestion of which albuminosity of the urine and suppression are the indices.

"When the primary reaction was violent, and the face was turgid, and the head symptoms severe, arteriotomy was performed and with benefit. In a few such cases, and when the patient was young, strong and full blooded, or where the dynamic congestions were so violent that the vessels yielded to the turgescence and impulse, and blood corpuscles without tube casts, or even but a haze of albumen, was present in the urine, the arm was opened, and free bleeding relieved the tension of the vascular system. In such cases convalescence was slow and unsatisfactory, but the immediate results had been beneficial.

"In general, the bowels responded easily to the action of mild purgatives; but a cluster of cases occurred about fifteen months after the commencement of the epidemic in which *croton oil* was required to follow the resolvent doses. *Hydrocyanic acid* was supposed beneficial in a few cases in abating primary irritability of the stomach, and being easily taken, may be borne in mind by the practitioner, as a variety of such resources are at times required. *Ether* was frequently attended with marked advantage in removing or abating the distressing symptom, hiccup, but it was also used as a diffusible stimulant, and when acceptable to the patient, is fully equal to brandy for that purpose."

Dr. B. remarks, that of all the auxiliaries which must be occasionally impressed into the service of the patient, by far the most important is *morphine*. It should never be given, however, when there is suppression or tendency to suppression of urine. Its beneficial effects are most visible and unqualified in those cases wherein the disease has been imperfectly aborted, in which, after a few doses of the *aq. acet. ammon.* and camphor water, it will induce a good night's rest, out of which the patient awakes free from disease. The maximum dose for an adult should never exceed eight drops of the solution of the acetate (one-fourth of a grain); and this dose should rarely be repeated within twenty-four hours. Morphine, we are told, is perfectly safe, while the urine is non-albuminous.

"The 'smouldering form' of yellow fever," according to Dr. B., "is best treated by rest, the recumbent position, cool drinks, and abstinence from any but the lightest food. The patient, however, should be closely watched, although interference is seldom required, the curative and conservative power of nature being adequate to the perfect restoration of health in almost all these cases. Inflammatory complications were treated on general principles; and in pneumonia the tartrate of antimony was borne well."

We must close our imperfect notice of this most interesting report with Dr. Blair's pathological definition of yellow fever.

"The proximate cause of yellow fever is an aërial poison which impinges against the exposed mucous surfaces, and there excites in the contiguous capillaries a specific irritation which extends itself in a varying degree to the whole capillary system. The chief anatomical results of this irritation are epithelial desquamation, exfoliation of basement membrane, and textural lesion of the capillary tubes, occasionally amounting to their entire disintegration. The morbid phenomena, supplementary to the febrile reaction, are the compound results of capillary congestion, extravasation, and hemorrhage, and of a circulation contaminated by retained excretions." D. F. C.

Art. XXV.—*On some points in the Anatomy of the Liver of Man and Vertebrate Animals, with Directions for injecting the Hepatic Ducts, and making Preparations.* By Lionel S. Beale, M. D., London. Illustrated with upwards of 60 photographs of the author's drawings. London, John Churchill, 1856: pp. 80, 8vo.

Since Mr. Kiernan, of London, gave the first satisfactory account of the intimate structure of the liver, in 1833, most of the living anatomists have investigated this subject. Nearly all agree as to the arrangement of the coarser structural elements, and the relative position of the different systems of vessels and the hepatic ducts; but opinion is much divided in regard to the exact arrangement of the secreting structure within the lobules of the organ.

Most authorities, including Henle, Handfield Jones, Hassall, Guillot, Kölliker, Hyrtl, Gerlach, and Carpenter, adopt the view that the bile secreting cells occupy the meshes of the intra-lobular vascular plexus, or in other words form an intertexture with the latter; some considering the rete of cells as a solid one, as Handfield Jones, and Kölliker, others describing intercellular passages for the escape of the bile, as Henle, Gerlach, and Guillot. From this intra-lobular plexus of bile-secreting cells, the hepatic ducts have their commencement, lined with epithelial cells, much smaller than the former cells.

Other authorities, including Krukenberg, Theile, Van der Kolk, Backer, Leidy, Retzius, Weber, and Lereboullet, adopt the view that the bile-secreting cells are contained within an intra-lobular plexus of tubes of basement membrane, from which the smaller hepatic ducts have their origin.

Dr. Beale, in the work above indicated, presents the results of his investigations into the intimate structure of the liver of vertebrated animals; and his observations and experiments bear the stamp of care and accuracy. After many experiments he has succeeded in making good injections of the hepatic ducts and the intra-lobular plexuses continuous with them, as was previously done by Berres, Hyrtl, Retzius, and others.

Dr. Beale's observations support the view that the bile-secreting cells are contained within tubes of basement membrane, about the $\frac{1}{1000}$th of an inch in diameter, and constituting the intra-lobular biliary plexus of Mr. Kiernan, within which originate the commencement of the hepatic ducts, which are not more than one-third the diameter of the biliary tubes.

The sacculi imbedded in the parietes of the larger hepatic ducts, usually considered to be mucus follicular glands, Dr. B. has discovered frequently to communicate with one another, and he considers them to be diverticula or minute gall-bladders.

The plates accompanying the work are photographic illustrations from the original drawings of the author. J. L.

Art. XXVI.—*General Therapeutics and Materia Medica: adapted for a Medical Text Book. With Indices of Remedies, and of Diseases and their Remedies,* By Robley Dunglison, M. D., LL. D., Professor of Institutes of Medicine, etc., in Jefferson Medical College of Philadelphia, etc. etc. With one hundred and ninety-three illustrations. Sixth edition, revised and improved. 2 vols. 8vo. pp. 544, 539. Blanchard and Lea, Philadelphia.

A critical review of a work which, like the one before us, has passed, within comparatively few years, through six editions, would be a labour entirely uncalled for. As a guide to the medical student in the acquisition of the principles and more important details of general therapeutics and materia medica, and at the same time, a trustworthy book of reference to the practitioner who would learn the leading results of modern observation and reflection on any of

the subjects of which it treats, the treatise of Dr. Dunglison could never have sustained its place amid the many contemporary treatises on the same branches of medical science, by authoritative writers, as well European as American, had it not been found in the main correct in its teachings, sufficiently copious as an elementary work, and withal clearly and skilfully arranged.

The work is too well known, and too correctly appreciated by those for whose use it was intended, to render any analysis of its contents necessary; we need only remark that in the present edition it has undergone a thorough and careful revision, while every new fact has been incorporated in it, so as to render it a faithful epitome of the actual knowledge of the profession on the branches of which it treats. D. F. C.

Art. XXVII.—*Researches into the Structure and Physiology of the Kidney.* By C. E. Isaacs, M. D., Demonstrator of Anatomy in the University, of the city of New York.

On the Functions of the Malpighian Bodies of the Kidney. By C. E. Isaacs, M. D., etc. New York, 1857.

"*Researches into the Structure and Physiology of the Kidney,*" and "*On the Function of the Malpighian Bodies of the Kidney,*" are the titles of two excellent memoirs by Dr. C. E. Isaacs, of New York, published in the *Transactions of the New York Academy of Medicine.* In these, Dr. Isaacs gives the results of a series of laborious and carefully made researches into the intimate structure and function of the kidney. In relation to the structure, his investigations generally confirm those of Müller, Bowman, Kölliker, and the later ones of Hyrtl, Gerlach, Leydig, and others. In opposition to Bowman, he agrees with Kölliker, Gerlach, and Leydig, in considering the Malpighian bodies as not only inclosed within the dilated extremities of the uriniferous tubules, but also to be invested with a distinct layer of cells. From numerous experiments performed on living animals, Dr. Isaacs attributes the principal agency in the secretion of the constituents of the urine to the Malpighian bodies, in opposition to the view of Bowman, and most physiologists, who consider that they merely separate the aqueous portion of the urine from the blood. J. L.

Art. XXVIII.—*On the Extent of the Pleura above the Clavicle.* By C. E. Isaacs, M. D., Demonstrator of Anatomy, etc.

This is the title of another excellent paper by the same author as the preceding, and likewise published in the same work. Dr. Isaacs has most industriously and carefully collected 100 cases in which he has ascertained the relative position of the pleura to the clavicle. As the result of these examinations Dr. Isaacs observes:—

"It has thus been shown that, in 100 cases, the pleura rose only to the upper margin of the clavicle, or very slightly above it, in 5 instances; that in 11 cases the right pleural sac extended across and to the left of the median line, making an average width of the pleura, at this point, of four inches; that a similar arrangement never was observed on the left side; that in 23 cases the pleura ran high, extending two inches and upwards above the clavicle, thus giving an average proportion of one in four; that in 14 instances this occurred on the right side, and in only 5 cases on the left, and on both sides in 4; that in one case only, the pleura attained the height of two and a half inches above the clavicle; and that the height and width of the pleura are both greater in the male than in the female. These facts, then, are of value to the physician, because the apex of the lungs corresponds with the concavity of the pleural sac above the clavicle, and it is important for him to ascertain its extent, if practicable, by auscultation and percussion. They are also useful to the surgeon, in the removal of tumours, and in other operations in this region." J. L.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *Anatomy of the Supra-Renal Capsules.*—The observations of Addison in regard to disease of the supra-renal capsules, and the investigations of Dr. Brown-Séquard in regard to their importance in the system, have invested these bodies with so much interest that the following remarks by Prof. Ludovic Hirschfield on their anatomy will be regarded as important:—

"Fixed solidity by numerous vascular and nervous prolongations to the inferior cava, to the aorta, to the renal and inferior diaphragmatic vessels, to the solar, renal, and diaphragmatic plexuses, the supra-renal capsules have no direct communication with the kidney, except by very thin areolar tissue. They never follow the displaced position of the kidneys. In new-born children they are so intimately connected with the kidneys that they appear to be a constituent part of the same. The volume of the capsule depends upon the age, being larger than the kidneys during embryonic life, almost as large as these organs in young children; far from increasing with age as the other organs, they rather diminish, so as to be only the twentieth or thirtieth part of the size of the kidneys. Variable in different subjects, their size is not the same with every adult, nor with old people, and their volume is not more considerable in the negro race than in the white race, as observation has many times shown. M. Cruveilhier mentions having found them voluminous in several aged women.

Considered as to their dimensions, they differ on the right and on the left. The left capsule has from 32 to 36 millimètres[1] in length, by 26 to 28 in breadth, that on the right side from 29 to 31 in length by 26 to 28 in breadth; their thickness towards the centre is from 3 to 5 millimètres. The weight, likewise, is not the same for each; sometimes the capsule of the right side is heavier than that of the left; at other times it is the contrary; on an average, however, it is from 4 to 5 grammes.[2]

In order to study the supra-renal capsules, anatomists distinguish two surfaces for each—the one anterior, the other posterior; and two borders—the one superior, the other inferior. The anterior surface presents near its middle part a groove, more or less transverse, destined to contain the largest of the capsular veins (the middle capsular). On the right side, the capsule is found behind the first curve of the duodenum, and in a depression on the posterior part of the inferior surface of the liver. On the left, it is situated behind the spleen, and the tail of the pancreas. The posterior surface, smaller than the anterior, corresponds on both sides to the semilunar ganglion and to the diaphragm. The inferior border or base, large and excavated, is moulded to the convexity

[1] One line English = 2.1166 millimètres French.
[2] One gramme French = 15.438 grains troy English.

of the superior extremity of the kidney, to which it adheres by loose adipose tissue. The superior border, sometimes curved, sometimes straight, at other times semilunar, is applied to the liver on the right, and to the spleen on the left side. In making a section of the supra-renal capsule from the inferior to the superior border, one appears to find a triangular cavity of a darker colour than the external surface, from the inferior part of which arises a small oblong crest. This cavity is only illusive, and results probably from the separation of the anterior and posterior walls after a previous rupture of the loose and delicate cellular tissue which united them, or, perhaps, as M. Rayer remarked, from the extravasation of blood following the rupture of the capsular veins. These contiguous surfaces are bathed in a humour, the consistence and colour of which vary with the subjects; of a dark hue in the adult, it is reddish and more abundant in the fœtus.

Structure.—There is in the composition of each capsule an enveloping membrane and a special tissue. 1. The enveloping membrane is formed of cellular tissue, mixed with elastic fibres. Its superficial surface is surrounded by a cellular fatty tissue, to which it is adherent by fibro-vascular prolongations; its deep surface sends into the substance of the body a multitude of prolongations, which divide it into numerous segments. 2. The substance proper, or parenchyma, is composed of two substances, juxtaposed, but quite distinct one from the other; one, external, is called the cortical substance; the other, internal, is called medullary substance.

(A.) Cortical substance, yellowish, and of rather firm consistence, with an average thickness of 2 millimètres. This thickness is divided into a considerable number of cavities by very thin lamellæ, united, and constituting a mass of conjunctive tissue, which enters into the composition of the cortical substance. Each cavity has a vertical direction in the substance, and a breadth which varies from 0.35 to 0.5 mil., and which is observed even to increase to 0.7 mil.

Some anatomists have found in these cavities utricles, each of which has an amorphous membrane, inclosing a substance of a granular nature, with which nuclei and cells are blended. According to Kölika, the cavities contain polygonal cells, from 0.14 to 0.27 mil., independent of each other, on both sides of the cortical substance, and united into cylindrical masses in the middle of this substance. Contrary to the opinion of D'Ecker, this same anatomist maintains that true utricles are found only in the internal portions, where they present themselves under a spherical or ovoid form, having a diameter from 0.5 to 0.7 mil., and containing masses of granular fat. The yellowish colour presented by the cortical substance is in consequence of the cells being entirely filled with granular fat, which gives them a great resemblance to the cells of the liver in a pathological state. These fatty granulations are added to the contents of the cells, formed in the normal state of minute granulations of a nitrogenized nature. In the deepest stratum of the cortical substance, that which borders on the medullary substance, and which is more coloured, are found vesicles containing pigment granules of a brown colour. (B.) Medullary substance. It is less solid, and has only a third of the thickness of the cortical substance. From its centre arises the middle capsular vein, and throughout its extent there is found a great number of nerves and vessels, principally veins, which explains its deep colour and spongy aspect. By decomposition, it changes quickly into a brown pulp, incorporated with blood; it is, therefore, necessary to study it immediately after death. It appears then under the form of thin lamellæ of cellular tissue, which are continuous with the septum of the cortical substance, forming a network of rounded and narrow meshes. One finds in this network a delicate granular substance, which contains pale cells of a diameter from 00.18 to 00.35 mil.

It contains some few fatty and pigmentary granules with nuclei and nucleoli. These pale cells have the appearance of ganglionic corpuscles, and remind one, to a certain extent, of the cells of the nervous system; but they cannot be confounded. The capsular bloodvessels, the capsular arteries, very numerous and variable in number, emanate from three sources, and are divided into superior, middle, and inferior. The superior capsular arteries arise from the inferior diaphragmatic arteries; they are very numerous and small, and reach the

superior part of the capsule, in which they are lost. The middle capsular, larger and less numerous, sometimes even single, arise directly from the aorta, and are distributed to the anterior and posterior parts of the capsule. The inferior capsular arteries come from the renal artery. All the capsular arteries anastomose freely with each other, being lost on the surface of the capsule.

The capsular veins do not vary generally so much as the corresponding arteries, their number being nearly always the same. On each side is found oftentimes a principal vein, the middle capsular, the size of which, especially on the right side, equals almost that of the external jugular vein. These veins, remarkable by their calibre, and by the absence of valves, course along the groove on the anterior surface of the capsules, and, having received in their course the ramifications proper to the capsule, some fine branches from the surrounding fat, and the inferior diaphragmatic branches, empty themselves, the right usually into the inferior cava, immediately under the liver; and the left, taking a longer course, opens into the corresponding renal vein. Independent of these principal veins, the supra-renal capsules have other venous branches, very fine and variable in number; they are the superior and inferior capsular. The former empty themselves into the diaphragmatic veins, the latter into the renal veins; it is not as yet shown positively that the capsular veins have no direct communication with the interior of the capsules, as has been maintained by some anatomists. The lymphatic vessels of the capsule are proportionately voluminous: some are superficial, others deep; those of the right side lead to the lymphatic glands, which are disseminated around the inferior vena cava; those of the left side conduct to the glands placed in front of the corresponding pillar of the diaphragm.

The capsular nerves, very numerous and small, arise like the capsular arteries from three sources, and are distinguished into superior, middle, and inferior. The superior come from the diaphragmatic plexus; the middle, more numerous, spring directly from the semilunar ganglia, and from the solar plexus; while the inferior are provided by the renal plexus. Bergmann mentions having seen nerves springing out from the semilunar ganglion, cross the border of the capsule, and return to the semilunar ganglion so as to form in their course small loops.

I have sought for this latter anatomical arrangement, but have never succeeded in finding it."—*Med. Times and Gaz.*, July 18, 1857.

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2. *Fibrin, its Nature and the Cause of its Coagulation.*—Regarding the nature and origin of fibrin, ZIMMERMANN repeats that he considers it as an excrementitious substance, exhibiting a certain stage in the metamorphosis of proteinaceous bodies, not any longer fit to serve in the nutrition of the organism. A small quantity of fibrin is regarded as a necessary constituent of the blood, but "the healthier the subject, the smaller the quantity of fibrin." By further oxidation, fibrin is, in the normal state, transformed into other excrementitious circumstances. "Sometimes, however," the author says, "the formation of fibrin takes place in so tumultuous and rapid a manner, that the transformation into excrementitious substances cannot take place; whence arises exudation of the accumulated fibrin, a process through which the blood is, for the time, purified of this substance. Later, when the cause for this abnormal crisis has ceased, the fibrin may again be absorbed and otherwise excreted. Such is the case in pleuritis, pneumonia, &c." (p. 181).

The coagulation is caused, according to Zimmermann, by the commencement of decomposition or putrescence; it is accelerated by the addition of substances in the state of transposition of elements, as pus or ichorous fluid from gangrenous wounds (*Nasse*), in the whole by all influences favouring putrescence; it is retarded, on the contrary, by such influences as retard or prevent decomposition. The putrescence causing the coagulation of fibrin does not take place in the fibrin itself, but in other constituents of the blood, and principally in the red blood-globules. The chemical constitution of the fibrin passing into the solid state is regarded as remaining unchanged, with the exception of transposition of its atoms, effected by the contact with a substance in the state of decomposition, analogous to the transformation of casein by the action of ren-

net.—*Brit. and For. Med.-Chir. Review*, July, 1857, from *Moleschott's Untersuchungen zur Naturlehre*, 1856.

3. *Assimilation of different Oleaginous Substances.*—The fact that some fatty substances, when eaten, are almost entirely excreted by the alvine dejections, while the amount of fat in the feces does not become increased by the moderate consumption of other fats, led Berthé to the examination of the quantity of fat excreted with the feces under the influence of various oleaginous matters administered to the same healthy subject in doses of from thirty to sixty grammes per diem. These experiments led the author to the inference that there is a point of saturation of the body for most of the fatty matters, from whence almost the whole amount of fat ingested passes unassimilated through the intestinal canal. This point is arrived at after about twelve days with olive and almond oil, and almost all vegetable oils; after about a month with butter, whale oil, and English purified cod-liver oil (*huiles de baleine et de foie de morue Anglaise, décolorées ou lavées*); while the use of the pure brown cod-liver oil (*huile de foie de morue brune et pure*) did not lead to an increase of fat in the feces, even when its use had been continued for more than a month. Berthé, therefore, proposes a division of the fatty substances into three groups: 1. Substances of difficult assimilation (olive, almond oil, &c.); 2. Assimilable substances (butter, whale oil, English cod-liver oil, and probably all animal fats); 3. Very assimilable substances (brown and pure cod-liver oil).—*Brit. and For. Med.-Chir. Review*, July, 1857, from *L'Union Méd.*, No. 62, 1856.

4. *On certain Physiological Phenomena connected with Parturition and Lactation in Bitches that have not been Fecundated when in Heat.*—Delafond directs the attention of the Imperial Academy of Medicine to several phenomena deserving further examination. He corroborates the observation made already by the great Harvey on doe-rabbits, and by Buffon on bitches, that the breasts of animals which have not been fecundated when in heat sometimes become turgescent, and secrete milk at the time when the parturition would take place if the animals had become pregnant. The author observed the commencement of the turgescence in bitches already two or three weeks before the term of the pregnancy would have elapsed. In addition to this, the author witnessed, at the period when the parturition would have occurred, enlargement and swelling of the vulva, and increased viscous secretion of the mucous membrane of the vagina; he even saw the animal in a restless state arranging a resting-place, as if for an expected process of whelping; a few days later he discovered the symptoms of milk-fever, and the bitch in this state allowed a puppy placed underneath her to suck her breast; she bestowed on it the same signs of affection as if it had been her own, and the young animal was evidently thriving by the nourishment it thus received. Leblanc, Roche, and Moreau, who took part in the discussion on the subject, related observations of their own of a similar nature, and Roche mentioned that Dubois had met with analogous phenomena in women.—*Brit. and For. Med.-Chir. Review*, July, 1857, from *L'Union Méd.*, No. 61, 1857.

5. *Erectile Apparatus of the Eye.*—Dr. C. Rouget, in a paper read before the Academy of Sciences (*Archives Gén.*, No. 3, 1856), brings forward new facts regarding the elementary structure of these parts. He refers to the eyes of birds, the principal mammals, and man. At the adherent border of the ciliary processes he describes a layer of bundles, which is the layer of circular fibres of the ciliary muscle. This in birds is formed, in front, of transversely-striped fibres; behind, of regular fibres, rounded, refracting, and analogous to the fibres of yellow elastic tissue, but containing here and there fine and regular transverse stripes. In mammals the circular layer is formed of bundles of dartoid fibre, smooth, and containing elongated nuclei, in the midst of which run plexiform divisions of ciliary nerves. Outside the annular ciliary muscle, in a kind of stroma of the choroid at the posterior edge of the ciliary region, are bundles of radiated ciliary muscle. These bundles, formed in birds of striated fibres, compose at least two muscular plains; one, external and poste-

rior, is inserted after a short course into the posterior region of the bony ring of the sclerotic; the other, covered at its origin in the choroid by the former, is prolonged anteriorly as far as the anterior margin of the bony ring where a portion of the fibres are inserted; whilst the remainder is attached to the membrane of Descemet, the true elastic tendon of the anterior plain of radiated muscular fibre. This is the muscle described by Crampton and others, who, according to Rouget, were deceived as to its origin and true meaning. The muscular apparatus of the iris is only the continuation of the deep plain of the ciliary muscle (circular fibres). In birds, the transversely striated bundles enter the iris obliquely, and, keeping in the course of this membrane a generally circular direction, cross each other more or less obliquely. In man, and most mammalia, the same disposition obtains as in that of the iris of birds. At the anterior edge of the ciliary muscle, the bundles of the deep layer of this muscle, continuing their direction obliquely transverse, penetrate the iris; and then, covering the external surface of this membrane, and entangling the vessels in their meshes, they cross each other more or less regularly, arrive at the edge of the pupil, and form the ring of circular fibres at the surface of the iris, the sphincters. The author then speaks at length of the mechanism of the parts, and of the functions they play in adapting the lens to vision, showing how the contractions of the circular bundles of ciliary muscle force the blood returning from the iris into the ciliary folds, which, by compression, affect the convexity of the lens.

MATERIA MEDICA AND PHARMACY.

6. *Employment of Amylene.*—M. Robert recently read, at the *Academy of Medicine*, an interesting report of a commission, composed of MM. Malgaigne, Velpeau, and himself, upon a memoir on amylene, submitted to the Academy by M. Débout. The report states that M. Débout's paper offers nothing new upon the subject, but confirms the observations already published by Snow, Giraldès, and Tourdes, establishing that amylene produces anæsthesia very speedily, without causing any painful sensation, or inducing that desire to cough, or expectorate, so often observed in the employment of chloroform. "During the entire procedure," says M. Débout, "the pulse remains large, full, and very rapid, the respiratory movements are deep, the skin warm, the face highly coloured. In one word, there is an absence of those signs which indicate that the new agent can readily exert any effect upon the phenomena of organic life."

The reporter, in order to judge of this favourable account of amylene, made a number of trials with it himself, employing Charrière's apparatus, which, by covering both nose and mouth, prevents a loss of the vapour. One of the remarkable properties of amylene being its slight solubility in the blood, its vapour must be breathed in as concentrated a manner, and as continuously as possible, under the risk of failing to produce insensibility, or of this being of too short duration. M. Robert believes that is the want of observing this precaution, and from merely administering it on a sponge, that some surgeons have failed altogether, or have had to employ very large quantities of amylene. He has employed it in 44 adults of both sexes. Most of the operations were but of short duration, such as opening abscesses or panaris, avulsion of nails, or amputation of toes; but some have been of a more important character, such as amputation of the thigh and arm, removal of the breast and of the parotid gland, and extraction of a calculus from the prostatic portion of the urethra, this last requiring a quarter of an hour. In none of these cases was any irritation of the mucous membrane produced. Most of them were rendered insensible in two or three minutes, a few not until six or seven; while in three, after ten or twelve minutes' inhalation had been tried in vain, chloroform had to be resorted to. The agitation which so frequently precedes the action of chloro-

form was not observed. The countenance became more and more flushed. The eyelids remained widely open, and the fixed eyes were frequently carried upwards, beneath the upper lid. The head was thrown backwards, and sometimes the limbs became extended and stiffened. The pulse very rapid, and, in one case, its intermittent and filiform character excited alarm. Respiration continued free, and the spasmodic closure of the jaws, with threatened suffocation, sometimes observed under chloroform, was never met with. It is an important fact, that amylene never gives rise to muscular resolution, and the insensibility it induces is of very short duration, unless the application be constantly repeated. After the operation, the restoration is rapid, the patient not continuing to suffer any uneasiness. Two young girls, however, exhibited a singular form of delirium, accompanied by sobbing and violent convulsive movements. One of them, subjected some time after to chloroform, exhibited the same symptoms. Thus, while amylene resembles ether and chloroform in its power of preventing pain, it differs from them in the rapidity and temporary duration of its action, and in not exerting any power in muscular contractility.

As to the question of danger, M. Robert observes that Dr. Snow's case has proved that, as in the use of chloroform, death may take place in consequence of a special predisposition of the economy of an unknown nature. The only question really is, whether there is less danger attaching to the employment of amylene. From various experiments he has performed upon animals, M. Débout concludes that there is; and he calculates that, if the dose of chloroform has to be doubled, to convert it from an anæsthetic into a toxical agent, that of ether has to be quadrupled, and that of amylene to be quintupled. The reporter, having repeated the experiments upon birds and rabbits, believes that this statement is accurate. Performing other experiments upon dogs, employing for that purpose a modification of Charrière's apparatus, he found that chloroform gave rise to the usual series of symptoms, from simple insensibility to complete muscular resolution, the animals always dying in the course of thirty or forty minutes. Under the use of amylene, the same symptoms were observed as in man, but a relaxation of the muscles was never attainable. Moreover, continuing the experiment, with the intention of killing the animals, these seemed to become habituated to its use, and recovered part of their sensibility. The inhalation was terminated at the end of an hour, and the animals soon began to walk, and were speedily restored. Having contrived an apparatus by which large quantities of amylene could be breathed in a concentrated form, the reporter at last succeeded in producing complete resolution of the limbs, and death followed in twenty minutes. While these experiments show that amylene is a much less powerful poisonous agent than chloroform, the reporter does not agree with M. Débout that it may, therefore, be more safely used in practice. It is, in fact, a leading point in the history of anæsthetics, that death has not supervened in man upon a successive and progressive evolution of the phenomena of intoxication, but that it occurs suddenly and unexpectedly, and in consequence of an unknown predisposition of the economy.

Amylene is, therefore, but another anæsthetic agent, to be placed side by side with ether and chloroform. Its action is prompt and of short duration, while its effects rapidly disappear. It is to be preferred in very short operations, when we wish simply to prevent or moderate the pain. Moreover, its not exciting irritation of the air-passages renders it a valuable agent when pulmonary lesions exist. It does not, too, give rise to vomiting or nausea, which are not infrequent after chloroform. This is important, especially in relation to children, enabling us to operate nearer their meal-time, and avoid the inconvenience of submitting them to long fasting, which they support very badly: nevertheless, vomiting does occur occasionally.

As the insensibility produced is of such short durability, and muscular contractility is only exceptionally influenced by it, amylene should not be employed for long and difficult operations, and especially those in which it is requisite to subdue the contractility of the muscles, as in dislocations, hernia, the diagnosis of abdominal tumours, etc. M. Tourdes has stated that muscular resolution may at last be obtained by a sufficient prolongation of the anæsthesia; but

independently of the inconvenience that might result from the absorption of such large quantities of amylene, and the troublesome uncertainty the surgeon is kept in, respecting the return to consciousness of his patient, it is by no means proved that such resolution would ensue upon a multiplication of the doses. Moreover, the reporter's experiments would seem to show that such repetition, providing the dose be not rendered excessive, tends to destroy the activity of the agent; while, when resolution was obtained, it was only shortly before death.—*Bull. de Thérap.*, May, p. 443.

M. VELPEAU, as a member of the Committee, observed that he was no great partisan of amylene, and does not think, so far as his own experience goes, that it is likely to displace chloroform. Against its employment are its detestable smell, alike annoying to operator and patient, the little constancy of its effects, and the necessity of employing an apparatus. As to chloroform, he believes that the amount of danger attending its use has been grossly exaggerated. During the ten years he has employed it in from 5 to 6,000 cases, for operations at different ages, and in both sexes, he has met with no accident whatever. No important accident has, indeed, occurred at any of the great Paris Hospitals. Moreover, how unjust, whenever a death occurs during or after an operation, to attribute it to chloroform! Employed with precautions, he does not regard it as a bit more dangerous than amylene, while it may be administered without any apparatus.

M. GIRALDES, at a subsequent meeting of the Academy (*Gaz. Méd.*, No. 21), gave a further account of his experience, having used amylene in the cases of seventy-nine children, varying from one to fourteen years of age, two drachms being in most cases the quantity employed. The time required for the production of the anæsthesia is usually about three minutes, but it varies much, both in different persons and in the same individual. No ill consequence has resulted, the anæsthesia taking place without reaction, or convulsions. In eight cases, muscular rigidity was observed; and when this occurs, the inhalations should be suspended a while, and free respiration permitted. In six cases vomiting occurred. The anæsthesia lasted for a very short time, but it may continue for eight or ten minutes, and is easily kept up. Chloroform is a more powerful agent, but it is also a more dangerous one. It may throw feeble children into a state of cadaveric resolution; and the anæsthesia it induces is often so prolonged as to endanger life. On other occasions it induces spasmodic closure of the jaws, rigidity of the muscles of the neck and thorax, and all the symptoms of asphyxia, rendering artificial respiration necessary. It generally gives rise to vomiting, or other signs of irritation of the digestive organs. After the employment of amylene sensibility rapidly returns, and the children seem in nowise to suffer from it. It may be given soon after meals, and to enfeebled children, in whom chloroform would prove dangerous. It thus has, like chloroform, its special indications, and frequently presents indubitable advantages.

Dr. KADLBURGER (*Wien Wochenschrift*, No. 19) relates the results of his experience of the employment of amylene in seventy-two cases of tooth-extraction, he applying it by means of a sponge to both mouth and nostrils. He states that the amylene he employs emits a smell of over-ripe pears, and is nowise irritating to the respiratory organs. From one to one and a half minute inhalation suffices for this purpose. The falling of the uplifted arm, as if wearied, is the signal for the discontinuance of the inhalation. No abnormal muscular movements, or closure of the jaw were produced. The patient readily opened the mouth upon being requested to do so; and during the extraction, most persons retained the consciousness of the application of the instruments. The pain, when felt at all, was very slight, and never at all approaching to the torture attendant upon ordinary tooth-drawing.

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7. *Anæsthetic Action of Carbonic Oxide.*—Dr. OZANAM considers that the results hitherto obtained by the use of anæsthetic agents concur to demonstrate the truth of the law that the whole series of carbonized bodies, volatile or gaseous, are endowed with anæsthetic power, and that they possess this power

in proportion to the carbon which they contain. He has therefore undertaken some experiments with carbonic oxide, carbonic acid, and cyanogen, and the results observed with the first-named gas are detailed in the present memoir.

The carbonic oxide is obtained by the action of sulphuric acid, aided by heat, upon oxalic acid; when the latter is resolved into carbonic acid and carbonic oxide, and the carbonic acid is removed by lime-water, while the carbonic oxide remains in the gaseous state. The experiments and observations were thirty in number, of which twenty-five were on rabbits and five on man. The author divides the phenomena produced by the inhalation of carbonic oxide into four periods—viz: 1. The prodromic period; 2. The period of excitement; 3. The period of anæsthesia; 4. Death or restoration of sensibility.

A pipe connected with a bladder filled with carbonic oxide was introduced into the mouth of a well-fed rabbit, whose nostrils were closed; an assistant pressed upon the bladder, and the animal, forced to breathe by the mouth, inhaled the gas mixed with atmospheric air. During the first five or six inspirations the animal made no effort; it was motionless and astonished, as if under the impression of a danger which it suspected but did not know, and of which it did not yet feel the violent effects. But at the end of fifteen to thirty seconds this preliminary period was succeeded by a stage of excitement: the animal leaped and made efforts to escape; then these voluntary movements were succeeded by very strong convulsions, contractions, throwing the head backwards, trembling, &c. During this period the circulation was accelerated at first from fifteen to twenty pulsations under the influence of the convulsive agitation; then it returned to its normal rate, which it soon quitted and became slower. The respiration, on the contrary, offered from the beginning a marked tendency to become slower. To the convulsive state suddenly succeeded the period of collapse or stupor; all movement ceased, the body fell back like an inert mass, the head hung down, the eye was widely open, the pupil dilated, the sight almost abolished, the four members were paralyzed, the urine passed involuntarily, the pulsation of the heart became more slow; the respiration also was less frequent. If the inhalations are prolonged, the respiratory act is weakened still more; it occurs only about once in five or ten seconds, by a general and jerking effort, resembling hiccough; but, prolonged to this degree, the anæsthesia becomes dangerous, and it should be narrowly watched, for the inspiratory nerves are almost paralyzed, and the animal approaches the state of apparent death.

The experiments made and recorded by Dr. Ozanam prove beyond a doubt the anæsthetic action of the gas, and he regards it as more energetic in its action than chloroform, but less prolonged in its operation; its effects are rapid, violent, and transient, so that an animal may pass in a few minutes from the state of apparent death to a normal condition. On the other hand, it possesses certain advantages over chloroform, in the absence of a strong, or penetrating, or caustic smell—a circumstance which renders the gas easily respirable by every person; while ether, chloroform, and the carburets of hydrogen have all a penetrating smell, which renders them offensive to many persons, and they are caustic when applied to the skin. It is also easily measured, owing to its permanently gaseous condition, which is not the case with chloroform and ether: their volatility varies under the slightest influence, as the summer, the heat of a room, or the vicinity of a stove, will cause a patient to absorb double the quantity of the vapours which would have been breathed if the circumstances had been different. The anæsthetic operation of carbonic oxide terminates in recovery from insensibility, or in death. When the inhalations are discontinued, the animal is abandoned to itself. During one to three minutes the anæsthesia remains absolute, and the animal might be considered dead, if auscultation did not still reveal the weakened sounds of the heart, and some rare inspiratory efforts. The ordinary life soon recommences, respiration is re-established, and the heart progressively resumes its normal rate, and sometimes slightly exceeds it. But occasionally the passage from stupor or apparent death to real death is sudden, unexpected, and similar in this respect to sudden death by chloroform: the heart and respiration, already very slow in their actions, cease at once and forever.

In case of poisoning by carbonic oxide, the antidote most likely to prove serviceable is ammonia; and Dr. Ozanam details two cases of rabbits poisoned by carbonic oxide, which were restored by the application of the vapour of ammonia.—*Brit. and For. Med.-Chir. Review*, July, 1857, from *Archives Gén. de Méd.*, Feb. 1857.

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8. *On the Effect produced on the Circulation by the Long-continued Action of Cold Water Externally.* By Dr. H. Bence Jones and W. Howship Dickinson, Esq.—Opportunities of making use of some douche and shower-baths of more than ordinary potency having presented themselves, the following experiments were undertaken, with a view of removing some of the uncertainty which now prevails regarding the effects of the outward application of cold water. These experiments are divided into three sections: 1st, on the general effect of the douche or shower-bath; 2dly, on the effect of the shower-bath at different temperatures; 3dly, on the effect of the shower-bath in different circumstances.

Section 1. The first experiment was made by a douche-bath, by which 225 gallons of water were allowed to fall upon the head for a quarter of an hour. By this the pulse was greatly relaxed in frequency and power, and it became irregular; at one period of the experiment the reduction amounted to 30 beats in the minute. The second experiment was made with a shower-bath delivering about 20 gallons of water a minute—upwards of 300 gallons in fifteen minutes. The results were similar to those obtained with the douche-bath, but were more marked. During the second minute, the pulse was found to be less frequent by 40 beats than it had been previous to the fall of water; and from the fifth minute to the fifteenth, when the experiment terminated, it was observed to be frequently intermitting and very weak. The third experiment was made with a still more powerful shower-bath, at Vienna. This delivered nearly 38 gallons of water a minute—upwards of 550 gallons in fifteen minutes; but the openings in the rose were very fine, and the shower was much spread. In the fourth minute the pulse was found to be imperceptible, and during the remainder of the quarter of an hour for which the bath was continued it was feeble and irregular. Afterwards the pulse was observed to be smaller and rather slower than it had been previously, but it was immediately restored by a warm bath. Thus it seems that a strong douche or shower-bath produces an excessive immediate effect upon the pulse. By the first shock it may be reduced in rate even 50 beats in the minute; it then recovers a little, but after four or five minutes, when the shivering commences, it again becomes reduced, and often is rendered quite imperceptible.

Section 2. The experiments in this section were made for the purpose of showing whether the effect varied with the temperature of the water. The most interesting are two which were made with the powerful shower-bath alluded to in Section 1, second experiment. In the first, the water was at 70° Fahrenheit. The pulse did not fall in rate for three minutes, although it lost much in strength and volume. When shivering commenced, at the end of the fourth minute, the pulse was imperceptible, and it was scarcely to be felt until the end of the sixth, and it remained weak and irregular until the termination of the experiment at the end of the tenth minute. In the second experiment the water was iced down to 50° F. The effect was much more rapid. During the first fifteen seconds the pulse was reduced at the rate of 38 beats per minute; this was followed by a reaction better marked than before, and the annihilation of the pulse, which followed the commencement of shivering, was much more complete and of longer duration.

Section 3. Some of the effects observed to follow the use of the shower-bath, taken under varying circumstances, are here stated. Two experiments were made: one at the baths at Ischel, in Austria, and one at the Prussian bath, at Vienna, where cold shower-baths were alternated with very hot vapour-baths. It was found that the increased action of the pulse produced by the exposure of the body to hot steam prevented that depression which would otherwise have resulted from the cold water. A converse experiment is quoted from Dr. Currie's "Medical Reports." An ague patient, who had derived advantage from the cold effusion during the hot stage of the fit, nearly died from the

alarming depression which resulted from the same application while he was in the cold stage.

The general conclusions are—

1. The usual effect of a strong douche or shower-bath is the immediate depression of the pulse. By the first shock of water between 64° and 68° F. the pulse becomes weak and irregular, and may be reduced in rate even fifty beats in the minute. After the first shock the pulse recovers a little, but remains weak until the secondary effect or showering comes on, when it becomes weaker and intermitting, and may be quite imperceptible. After ten to fifteen minutes the pulse remains very small and weak, and shivering continues whilst the experiment lasts.

2. If the shower-bath is a small one (eight gallons), and the person taking it in good health, no great difference is perceived in the pulse whether the water is hot (110°) or warm (74° F.). If the water is very cold (47° F.) the pulse becomes smaller, but the rate is not affected.

With a shower-bath giving twenty gallons per minute a difference of twenty degrees (from 70° to 50° F.) causes a great difference in the shock. The difference in the after-effect, or shivering, is not so marked. The depression of the pulse when the shivering comes on is more continuous with the colder water, and is more manifest up to the end of the experiment.

3. When the pulse is raised above, or depressed below, its healthy standard, the shower-bath or douche produces very much less or a much greater effect than would be produced by the bath under ordinary circumstances.

As it seemed possible that a part of the reduction of the pulse might be due to the action of the cold water upon the capillaries and the radial artery in which the pulse was felt, a set of experiments were made in which the forearm and hand were exposed to temperatures varying from 25° to 124° F. The results of these experiments may be thus stated:—

1st. When one arm is in water at 50° and the other in air at 46° F., no difference in the pulse is observed in fifteen minutes.

2d. When one arm is in water at 110° and the other in air at 46° F., little if any difference could be felt in the same time.

3d. When one arm is in water at 44° and the other in water at 107° F., there was the same result in the same time.

4th. Even one arm at 33° and the other at 112° gave no result.

5th. Still lower and higher temperatures, 25° and 115° F., did not give any decided result in fifteen minutes.

6th. The douche-bath on the arm and hand, at 42°, produced no greater effect on the pulse than still water at 44° F.

Hence, generally, it follows, that no part of the effect produced by the shower-bath on the pulse, depends on the action of the water on the hand and forearm in which the pulse is felt.—*Proceedings of Royal Med. and Chirurgical Society*, April 14, 1857.

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9. *The Bittera Febrifuga as an Antiperiodic.*—The practitioners of Martinique having transmitted to Europe highly favourable accounts of the febrifuge qualities of a plant found there, the Minister of Marine has directed that its efficacy should be tested at the different naval hospitals; and in this paper M. Delioux furnishes an account of the results of the trials he has made of it at Brest. The plant is one of the Rutaceæ, and has been termed *bittera febrifuga*, from the English term "bitter-ash," by which it is popularly known at Martinique. Its active principles are resident in a bitter resinoid, and in a bitter principle which it is proposed to term *bitterine*, and which is very analogous, if not identical, with *quassit* or *quassine*, obtained by Wiggers from *quassia amara*.

The bittera may be administered in the form of powder, infusion, or extract, or the bitterine itself may be given in pills. The intense bitterness of the drug disinclines some patients to it. M. Delioux has, as yet, only given it in the form of extract, made into pills, administering from 10 to 15 grains, in divided doses, during the pyrexia. This, he believes, is a better mode of giving this and other bitter tonics used as succedanea to quinine, than prescribing them, as in the case of quinine itself, in a large dose just before the paroxysm. Al

though declared in the Antilles to be an almost infallible febrifuge, sometimes superior to quinine itself, M. Delioux believes it to be, in this respect, not only much inferior to quinine, but also second even to arsenic—superior to the latter though it be in the tonic power it exerts upon the digestive and general system. After this, however, it should be ranked as one of the best succedanea; and, doubtless, as a good bitter tonic it is destined to play an important part in the relief of many organic and functional debilitated conditions, and especially in the anæmic and cachectic states resulting from paludal intoxication. In a great number of æsthenic diseases, in anæmia, in chlorosis, in convalescence from fever, in exhaustion from hemorrhages or discharges, and whenever it is desired to impart tone to the digestive and assimilatory powers, the bittera is indicated. In some forms of gastralgia, as far as M. Delioux's experience has gone, he believes it will prove very useful.—*Med. Times and Gaz.*, April 4, 1857, from *Bull. de Thérap.*

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10. *Therapeutic Employment of Cocoa-Nut Oil.* By Dr. Pettenkofer.—For some years past, cocoa-nut oil has been employed in the Munich Hospitals in place of lard, as being far less liable to become rancid. It is especially suited for frictions, as the warmth of the skin speedily renders it completely fluid and absorbable, while lard remains unabsorbed, soiling the linen and clothes, and, in spite of all cleanliness, giving rise to a rancid smell. Ointments of iodide of potassium made with lard, even when quite fresh, speedily become more and more yellow, pure iodine in place of the iodide being present. Made with cocoa-nut oil, there is not the least change of appearance for two months or more. The still frequent employment of butter in eye-salves might advantageously be replaced by that of cocoa-nut oil. Various ointments, too, if prepared with this, might be kept ready prepared in the shops, as they would not then undergo the changes they do when made with lard. Moreover, this oil will combine with a third more of water or other fluids than lard will, a matter of importance for the combination of extracts and solutions of salts. It is of a moderate price, usually of a very white colour, and possesses a peculiar smell, which is not disagreeable to most persons.—*Med. Times and Gaz.*, July 18, from *Buchner's Repert.*, Bd. v.

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11. *Physiological Action of the Disulphate of Quinia.*—Dr. H. Ranke states (*Med. Times and Gaz.*, May 30, 1857) that he has found, as a uniform result of five experiments which he has made on three healthy individuals, that the disulphate of quinia diminishes the quantity of uric acid in the urine.

"The importance," he observes, "of this action of the drug, if it prove to be constant, is obvious. Hitherto, our notions on the physiological action of quinia have been exceedingly deficient, and so is our knowledge of the real nature of ague, for which quinia is such an admirable remedy. Now, in ague there is, according to all observers, a considerable increase of uric acid in the urine, and, moreover, the spleen, the organ principally affected in ague, contains, according to Scherer, normally, some uric acid. Is it not possible that, by the study of the physiological action of quinia, we may in time be able to throw some light even upon the nature of ague and the process of its cure?

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12. *Pyrophosphate of Iron and Soda in Anæmia.*—Messrs. Follet and Baume call attention (*Gazette Hebdomadaire de Méd.*, May 20) to the advantages of the pyrophosphate of iron and soda as a remedy in anæmia, and the diseases of which that condition is a symptom, or with which it is coincident. They state that, in 1849, Dr. Leras presented to the Academy of Sciences a work on "The Action of the Gastric Juice on the Preparations of Iron employed in Therapeutics;" and that in 1855 he presented to the Academy of Medicine a memoir in which he expressed the following opinions:—

"1. The ferruginous preparations employed in therapeutics are all more or less precipitated, or transformed into oxide of iron in the stomach.

"2. The pyrophosphate of iron and soda is an exception.

"3. This salt seems destined to take a place among the most efficacious preparations of iron."

Dr. Leras prevailed on several medical men to try on their patients a solution of pyrophosphate of iron and soda, prepared by himself, and the results have just been published by Messrs. Follet and Baume in a "Report on the Clinical Service at the Asylum of St. Athanasius during the year 1856." They say that even in cases where there is no longer any hope of curing the mental maladies, there may still be a means of retarding the physical decline, which supervenes in almost all the patients in the same form, by a gradual failure of general innervation—a decline which is shown frequently in the decomposition of the blood, from which result those dropsies which are met with in all the tissues. In such cases, all the efforts of medication ought to be directed to the reconstitution of the blood, and when this result is obtained, it is not unfrequently seen that the system recommences its functions, a reaction in the inverse direction of the disease sometimes taking place; it is the prelude of an unexpected cure.

Messrs. Follet and Baume know no tonics which act so promptly and favourably as the pyrophosphate of iron and soda, prepared by M. Leras, in a liquid form; it is easy to administer, rapidly absorbed, and does not produce fatigue to the digestive organs. They then give the particulars of seven cases in which the medicine was tried during the year 1856; the results are certainly remarkable.

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13. *On the Preparation and Therapeutical Employment of Subcarbonate of Bismuth.*—The following is the mode of preparation of the subcarbonate of bismuth described by M. Hannon, Professor at the University of Brussels. The bismuth is first purified by melting this metal in powder with ten times its weight of powdered nitre. After cooling, the metal is again powdered, and mixed with ten times its weight of nitre, and after a second fusion the bismuth may be considered as entirely free from the arseniurets and sulphurets which it almost always contains. Then three parts of nitric acid are put into a retort, and one part of pure bismuth is added. When the reaction is complete, about a third of the liquid is evaporated, then the solution is poured drop by drop into a solution of carbonate of soda, and a white precipitate is obtained, which is subcarbonate of bismuth. The precipitate, after having been washed five or six times with distilled water, is thrown upon a filter, and washed again to remove the last traces of carbonate of soda. It should be preserved in well-stopped bottles. The physiological properties of the salts of bismuth are very little known, for the simple reason that the subnitrate is the only salt which has been employed in medicine. The operation even of this salt is not well understood, as its insolubility offers an obstacle to the observation of the physiological phenomena which might have been observed in the other salts of bismuth, such as the citrate, the tartrate, the acetate, or the carbonate. It is also the insolubility of the subnitrate which renders it inefficient in the greater part of the cases in which it is indicated; and it also occasionally produces a very inconvenient sensation of weight at the stomach. The subcarbonate is soluble in the gastric juice, its action is rapid, it produces no sensation of weight at the stomach, it rarely constipates, colours the stools less than the subnitrate, and may be employed for a long time without oppressing the stomach. The action of the subcarbonate appears to be sedative during the first days of its employment, and subsequently to excite all the phenomena which result from the action of tonics.

As to its therapeutical action, it may be noted that all cases of gastralgia consecutive upon phlegmasia of the digestive passages, cases in which the tongue is red and pointed, and cases in which the digestion is laborious and accompanied with putrid or acid eructations, or in which there is a tendency to diarrhœa or spasmodic vomiting, demand the employment of the subcarbonate of bismuth. This salt is also required in the vomiting of children, whether caused by dentition or succeeding to frequent fits of indigestion, and in the diarrhœa of weak children, especially when occurring at the time of weaning. One great advantage possessed by the subcarbonate of bismuth is, that it neutralizes the acids in excess which are found in the stomach. The subnitrate, as is well known, fails always in this respect. In all the cases where the subcarbonate has been taken, the pain in the digestive passages is first found to

disappear; then the eructations cease, together with the vomiting or diarrhœa; the digestion becomes less and less laborious, the tongue gradually receives its normal form and colour, and, if the use of the subcarbonate is continued, the appetite increases from day to day, the yellow tint of the countenance disappears, and the face becomes coloured at the same time as it ceases to be shrivelled.

The subcarbonate of bismuth is perfectly insipid, and excites no repugnance. It is given before meals. Adults take it in a little water, and children in honey. It may also be made into lozenges. The dose for adults is from one to three grammes, taken three times a day, in increasing doses.—*Brit. and For. Med.-Chir. Review*, July, 1857, from *Bull. de Thérap.*, Feb. 15, 1857.

14. *A New Principle of Colchicum autumnale.*—M. Oberlin has just communicated to the Academy of Sciences at Paris some observations on the Colchicum autumnale, from which he has extracted a neutral crystalline principle which he calls *colchicéine*, and which differs from *colchicine*, a complex and uncrystallizable product. The properties of colchicéine are, to crystallize very easily in pearly laminæ, and to be almost completely insoluble in water, but to communicate to this fluid a slight bitterness, which increases sensibly when it is boiled. At this temperature a notable part of the product is dissolved, but is deposited immediately after cooling. The solvents of colchicéine are alcohol, ether, methylated spirit, and chloroform, which contract, when mixed with it, a very intense and persistent bitterness. The alcoholic solution of colchicéine is coloured by the addition of bichloride of platinum, but no precipitate is formed. Pure concentrated nitric acid dissolves colchicéine, and becomes coloured of a very intense yellow tint, passing into a violet colour, then to a deep red and a clear red, and finally returning to its primitive yellow colour. Concentrated sulphuric acid forms with it a solution of a very intense yellow colour, which is preserved even when it is diluted with water, and brownish flocculi are formed in it. Hydrochloric acid dissolves it with a clear yellow colour. The acetic acid also dissolves it, but without change of colour. Colchicéine is soluble in ammonia, and crystallizes by evaporation in the air; and it dissolves in caustic potash. It is unalterable in the air; it has no effect upon turmeric paper or litmus paper; exposed to heat, it first softens and afterwards fuses at 155° (Cent.?). The elementary composition of colchicéine is C62, 83+H6, 60+N4, 19+O26, 38=100, 00.—*Ibid.*, from *L'Union Méd.*, Jan. 10, 1857.

15. *Ether and Chloroform Gelatinized.*—Professor Rusponi has succeeded in turning ether and chloroform into gelatine, by shaking them with white of egg in a closed receiver. The compound obtained with the ether is semi-transparent; with the chloroform it is white and opaque. This gelatine is soluble in water, and may be spread on linen in the form of a poultice. It will likewise mix with morphine, cantharidine, conicine, &c., and may thus become of great therapeutical use.—*Lancet*, August 8, 1857.

16. *Manganese cum Potassa.*—Mr. Weeden Cooke (*Lancet*, Aug. 8) extols this new preparation as a valuable caustic in cancer. "This caustic," he says, "contains a very large quantity of oxygen, and would seem to act by imparting this to the tissues, thus producing a chemical combustion. The pain produced is much less than that of any other caustic, and in some instances after the first minute or two there is no pain at all, and I have observed no after ill consequences. This 'manganese cum potassa' caustic is a dark green powder, and may be applied very readily by means of a small pepper castor. A thin coating of it will remove instantly all unpleasant odour from the ulcer, and when used for reducing the exuberant growth, must be applied in a layer as thick as the tissue to be destroyed. By dropping a few drops of water upon the powder after it is applied, it will form a paste, and adhere to the part, after which simple dressing may be applied. By means of carrot poultices the eschar drops off in three or four days. If necessary, the manganese is reapplied in the same easy way until the diseased mass is all destroyed, and the subjacent healthy tissues granulate and cicatrize by means of a slightly stimulating

lotion of chlorate of potash. Velpeau has a high opinion of the sulphuric acid, but could not succeed in making a paste with it to limit its action. This new combination of manganic acid and potassa will, I hope, attract his attention, and supply him, as well as my own countrymen, with an efficacious caustic, convenient for application, less painful than all others, and free from injurious effects upon the constitution. The well-known antiseptic effects of manganic acid suggests its use in all foul and phagedænic ulcers, and I have no doubt that large crops of warts may be conveniently removed by its agency."

17. *Corrosive Sublimate and Collodion as an Escharotic.*—Dr. Macke states (*Medicinische Cent. Zeitung*), that he has used with advantage an escharotic composed of one ounce of collodion to one drachm of corrosive sublimate. He has used it principally in nævi, and small excrescences which patients wish to get rid of without the use of the knife. It is applied with a camel's hair pencil, dries rapidly, and cannot be rubbed off easily. Applications of cold water may be made in cases where the inflammation runs high, without interfering with the action of the caustic. The eschar is thin, and falls off after three or mostly six days; pain is very slight, and the author has not found that any absorption of the bichloride takes place.—*Brit. Med. Journal*, Aug. 1, 1857.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

18. *Etiology of Diabetes. Diabetic Sugar not the same as the Sugar produced in the Liver in Health.*—Dr. G. Owen Rees, in his valuable Croonian lectures, recently delivered before the Royal College of Physicians, makes the following interesting remarks on this subject:—

"According to M. Bernard, we have not now to determine how a substance, foreign to the healthy constitution of the blood, becomes engendered in the system, but merely to inquire into the causes producing, on the one hand, an over-activity in the sugar-forming action of the liver, or, on the other, the diminution of the destructive power apparently possessed by the blood in health over that sugar when it has mingled with the circulating fluid.

Now, all this is clear enough were the sugar secreted by the liver, and that produced by injuring the base of the fourth ventricle, identical with that existing in the urine of true diabetes. This, however, is not the case, and we are not, therefore, so nearly about to unravel the difficulty as we might, at first, be inclined to believe.

About two years ago, I took the opportunity of obtaining blood from the hepatic veins of a dog, in order to determine the presence of sugar; for, like many others, I was, at first, a little incredulous. By the assistance of my friend, Mr. Hilton, this was effected without much difficulty.

On examining the blood obtained in this way, I found, it is true, that it yielded me sugar; but there was a peculiarity in the reaction of the tests, which led me to suspect I was not dealing with the same sugar as that contained in the urine of diabetes. It was quite impossible for me at the time to undertake a chemical investigation of the subject, and I was not sufficiently satisfied with my results to venture on publication. Some months ago, I mentioned my suspicions to my friend, Dr. Pavy, who has thrown much light on this interesting subject, and he told me that the same doubt had occurred to him some time since, and he immediately showed me from his note-books that he had worked the question out very satisfactorily, though he had not published on the point. Having Dr. Pavy's permission to do so, I will now detail the results of his investigations. It appears that the principal point of difference between these sugars consists in the greater facility possessed by the hepatic sugar, and by the sugar of *artificial* diabetes, of undergoing destruction by contact with animal tissue. This has been shown by an experiment made on

the sugar of *artificial* diabetes, comparing the result with that obtained by similarly treating grape sugar and *true* diabetic sugar. The experiments were conducted as follows: Three vessels were taken. In the first, a quantity of pounded liver, obtained from a healthy dog, was placed with a solution of the urine of artificial diabetes; the specific gravity of the solution was 1045. In the second vessel was placed pounded liver with a solution of common grape sugar, of specific gravity 1040. In the third was placed pounded liver with a solution of extract of true diabetic urine, of specific gravity 1040. The pounded liver was used (as any other animal matter might have been) merely to induce changes in the elements of these saccharine principles by its presence. The three mixtures were now set aside for nine days. At the end of that time, on submitting them to examination by Barreswil's solution, it was found that the artificial diabetic sugar had entirely disappeared, while the reactions were obtained in all their completeness from the two other solutions. Experiments made with the same solutions, substituting blood for pounded liver, led to the same results, showing a power of resisting decomposition on the part of grape sugar and *true* diabetic sugar far exceeding that existing in sugar obtained by the production of diabetes artificially.

There seems little doubt that the sugar of diabetes is a higher quality of the principle, and that it can preserve its atomic arrangement with far greater force than the hepatic variety. A power, however, seems to reside in the blood, which, after *some length of time*, eventually destroys, not only hepatic sugar and that of diabetes artificially produced, but even that of true diabetes mellitus. Thus, Dr. Pavy's experiments show that if the blood taken from a diabetic be allowed to coagulate, and the serum then be separated from the crassamentum, we can detect scarcely any evidence from the latter after a very long exposure. In the serum, however, it can be detected in quantity till decomposition is thoroughly set in. For some considerable time, both crassamentum and serum give full evidence, however, which contrasts strongly with the reaction of blood taken fresh from the right ventricle in health, and which contains *hepatic* sugar, for here the sugar disappears almost immediately the separation into serum and clot is completed. It is almost certain that when we produce the artificial diabetic state by operation, we obtain in the urine the hepatic sugar of the liver. It is also proved that this sugar of *artificial* diabetes is not the same as the sugar of *true* diabetes.

Now, of course, were these sugars identical, we might consider true saccharine diabetes as a disease in which the sugar-forming property of the liver became abnormally active; or, on the other hand, a disease in which normal sugar was formed in the liver in usual quantity, but that the blood had lost the power of destroying it when so formed, and that it, therefore, appeared in the urine.

The results I have detailed place us, however, in a very different position. We know now that true diabetic sugar is destructible only with great difficulty, and that it is not the same as ordinary hepatic sugar. The question will then arise—Are we to regard the sugar of diabetic urine as a modification of that poured into the blood by the hepatic veins in health, or, on the other hand, as a product of disease bearing no relation whatever to the sugar of the liver?

To those who have studied the subject of sugar in its chemical relations, who are acquainted with its varieties and the facility with which these are convertible into each other by the most simple processes, there will be no difficulty in believing that the sugar of diabetes may be easily derived from that produced in the liver in health. Late experimenters on the sugars obtained from the vegetable kingdom have shown how easily transmutations are thus effected, and chemical properties developed or abstracted by simple contact with materials apparently possessing anything but chemical activity. No one can fail to be struck, for instance, with the curious fact that the sugar contained in fruits possesses a certain action on light, influencing polarization, which action is precisely reversed in the sugar obtained *by crystallization* from the very same source. Thus, the gummy kind of sugar obtained from grapes possesses the property of *left-handed* circular polarization; but if we allow this sugar to lie exposed, a kind of imperfect crystallization occurs throughout the

mass; and if we collect the granular crystals so formed, we find we have in these a sugar differing materially from that originally extracted from the fruit. Its chemical constitution is not the same. Its constitution is $C_{12}\ H_{14}\ O_{14}$, instead of $C_{12}\ H_{12}\ O_{12}$; and when examined optically, it is found to possess the property of *right-handed* circular polarization. The change appears to be effected here by some constituent of the vegetable juice exercising its influence as crystallization goes on—probably the acids play an important part. Now, the liver, owing to some diseased action, may be supposed, in diabetes, to produce a sugar differing from that of health—a sugar which cannot be destroyed by the changes taking place naturally in the blood—changes rapidly affecting and destroying healthy hepatic sugar.

The phenomena of diabetes mellitus are, then, not quite so simple as the experiments and discoveries of Bernard would, at a first view, make them appear; and we have yet to determine the causes in action for the formation of this abnormal sugar. Does the presence of a different ferment interfere—even as we observe catalysis productive of varying results out of the body—may not an analogous action be going on in the liver? and, if so, what may be the nature of the ferment productive of disease, and whence is it derived? Are we to look to the portal blood for the ferment, or controlling influence which forms this less destructible sugar? And is it owing to this diseased state of blood that the liver, even though unaffected, is unable to cause the changes occurring in health?

But we need not have recourse to the theory of a ferment. The portal blood may present such principles to the liver as are only convertible into the *true diabetic sugar*. So far as we can yet determine, then, the whole phenomena of diabetic disease may eventually be traced to an abnormal state of the bile, gastric-juice, and pancreatic secretion, any one or all of which may interfere with the formation of healthy products in the portal blood, and so overpower a healthy liver in the discharge of its office. Analogy would certainly, however, rather direct us to conclude that in diabetes the function of the liver becomes altered under the influence of some cause as yet unknown. Bernard has proved that the organ in health has a very strong transformative action on grape sugar; and so powerful is this, that we should almost be entitled to conclude, even in the event of the portal blood bringing diabetic sugar, ready prepared, into the hepatic circulation, that it would be metamorphosed by the liver into normal *hepatic* sugar before it could reach the cava through the hepatic veins.

These results, then, taken together, render it probable that we are to look for the cause of diabetes mellitus in a disturbed state of the hepatic function, not in an increase of *natural* action, but in an action varying in *kind*. We see that in health the liver would reduce proximate animal principles to a normal hepatic sugar, and, in the perversion of force occurring in diabetes mellitus, we have a product given us approaching in character, it is true, to the normal sugar, but by no means identical with it. There is great facility for theorizing with respect to the agencies in operation in affecting this change of action. As vegetable juices contain principles which, by simple contact, can alter the chemical and optical qualities of the sugar first generated in the fruit, how easy to believe that the elaborate fluids contained in the several parts of the circulatory system of the liver may do the same. We know that acids are active in the vegetable kingdom—we know that the liver substance is acid—may not an over acid state cause the production of this abnormal sugar? or may not even a too slow circulation through the organ (by allowing too long contact with acid matter) bring about disease? These are questions requiring much consideration."—*Lancet*, May 30, 1857.

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19. *Diabetes; Concretion on Nervus Vagus.* By Hr. Nyman.—Early in the morning of the 24th of August, I was requested by Dr. Zanteson to accompany him to a Notary D., who had been taken suddenly and violently ill. On our arrival we found the patient in a semi-comatose condition, with a small, weak, and rapid pulse, cold extremities, and stertorous respiration. He was unable to give an account of himself, and in answer to questions uttered merely some

unconnected words. Dr. Zanteson now explained that the patient had long suffered from diabetes mellitus, for which he had last spring been under treatment, at the Seraphim Hospital, and had afterwards, during the summer, taken the waters at Carlsbad, whence he had lately returned home. From a person living in the next room to Notary D., we learned that after his return from Carlsbad, he had lived rather freely, and had, for the last two days, complained of feeling rather indisposed, but that he had, nevertheless, constantly attended to his business, and was sprightly and cheerful. On the afternoon of the 23d, he, for the first time, complained of headache and oppression of the chest, and of shiverings alternating with febrile heat. However, no one had been with him during the night, and in the morning the chamber-maid first found him in the state above described.

At a time when cholera was prevalent, it was natural to suspect that this sudden and violent attack was one of that disease. A glance at the patient was, however, sufficient to dissipate such an idea. The face was bloated and red; the patient had had neither vomiting nor diarrhœa; on the contrary, he had for several days suffered from constipation; the urine was voided in considerable quantity; there was neither cramp, hoarseness, nor any other symptom indicative of cholera. We now made an accurate physical examination, but could discover nothing abnormal in either the thoracic or the abdominal cavity; still all the symptoms indicated a speedy and fatal termination of the case. Death occurred in the afternoon of the same day. Dr. Zanteson now informed Professor Huss, under whose care the deceased had been in the hospital, of his death, who requested us, at the *post-mortem*, accurately to examine the nervi vagi, in reference to the diabetes, under which the patient had laboured.

The autopsy took place on the following day, the 25th of August, Doctors Sydow, Granberg, and Lundberg, also being present.

The state of the principal internal organs was as follows: The membranes of the brain were highly congested with blood; there was no effusion either under the arachnoid or in the ventricles. In the summit of each lung were some few scattered crude tubercles; there were no miliary granulations. The heart, relaxed and flaccid, contained a little coagulated blood in the right ventricle. The pleura and the pericardium were slightly adherent. The blood in the larger thoracic vessels was dark and thick. The stomach, which was considerably dilated, contained some spoonfuls of dark fluid, in which half-digested white bonum magnum plums were floating. In the small intestines was no trace of fluid; the mucous membrane was covered with a little yellowish brown mucus. The transverse and descending colon were filled with masses of solid excrement. The kidneys were hypertrophied and congested with blood. The ureters were dilated. The bladder considerably enlarged, contained more than a pound of urine. The liver was of natural size; its substance was flaccid and loaded with blood.

After we had examined the above-named organs, we commenced the dissection of the nervi vagi. On the left nothing abnormal could be found, but, on examining the right, we met, in the thorax, immediately behind the bifurcation of the bronchi, a calcareous mass of the size of a hazel-nut, which was under the entire trunk of the nerve, and appeared to have exercised considerable pressure upon it.

This morbid condition now observed by us for the third time, in persons dead of diabetes, gives additional support derived from pathology to the truth of the ingenious Bernard's well-known physiological doctrine. It may, indeed, be objected that the nervus vagus has other functions than the formation of sugar in the liver to perform, and that these functions also ought to be disturbed by a mechanical pressure on the vagus. But is not this, perhaps, in some measure the case in diabetes mellitus? The voracity at least accompanying this disease, seems also to depend on irritation of the vagus, which presides over the secretion of the gastric juice.

Before closing this statement, I may be permitted to suggest a theory of the origin of these calcareous concretions on the vagus. The calcareous concretions we met in this case, bore at first sight a striking resemblance to a bron-

chial gland, and such it was found on close investigation really to be. Was, then, this ossified bronchial gland anything else than a blighted tubercle? At least we may thus easily and naturally explain, not only the formation of such calcareous masses on the vagus, but also the near connection, so long observed, between tuberculosis and diabetes.

Hr. Huss mentioned that the person in question was under treatment in the hospital for four or five months, in the latter part of last year, and the beginning of the present. The urine then contained six or seven per cent. of sugar. The appetite was inordinately great. The quantity of urine often amounted to about three quarts (5 à 6 kannor) in the twenty-four hours. The urine contained no uric acid. Both ordinary and extraordinary means were employed; among others, yeast. Astringents with opium somewhat diminished the quantity of urine. By the advice of Hr. Huss the man went in the beginning of summer to Carlsbad, although the prospects of improvement were not great, the uric acid being absent. On his return from Carlsbad, he visited Hr. Huss, and looked tolerably well, but in other respects circumstances were unchanged. It is universally admitted that there are several forms of diabetes which cannot be distinguished until after death. The present case, so strongly confirmatory of Bernard's views, is an interesting example of one of these forms. With respect to the nature of the concretion, Hr. Huss fully concurred in Hr. Nyman's opinion. It is, however, remarkable, that only the formation of sugar should be disturbed by such pressure on the vagus, and that other functions dependent on that nerve, as those of respiration and digestion, should not be in any way interfered with. The insatiable appetite in this case ought also certainly to be regarded as a consequence of the affection of the vagi.

To a question by Hr. Carlson, whether uric acid was also absent in the two cases previously communicated to the society, in which, on *post-mortem* examination, concretions were found on the vagus, Hr. Huss replied that he was not able to give any positive information on the subject.—*Dublin Hospital Gaz.*, July 15, 1857, from *Transactions of the Swedish Society of Physicians.*

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20. *On the Mortality from Eruptive Fevers at Different Periods of the Year.*—Dr. Tripe read a paper (June 1) before the Epidemiological Society, the main results of which were as follows: 1. That smallpox presents two periods of depressed, and two of elevated mortality; the first period of elevation occurring in January, and the second at the end of May, or early in June; the former being the highest; the first period of depression being at the end of March or early in April, and the second in September, the latter being the lowest; and that, therefore, smallpox is most fatal in winter, next in summer, and least fatal in spring. 2. That a series of cold springs is attended with comparatively a large mortality, and that the period of highest mortality coincided with a temperature of less than 60° Fah., and the lowest of above 46° Fah. 3. That the highest average mortality in the seventeen corresponding weeks of 1840–56 was more than double the average lowest mortality. 4. That measles has only one period of highest and one of lowest mortality, the former occurring in December, and the latter varying in different years; but the rate of death in spring is much smaller than in any other quarter. 5. That the rate of death was greater in the series of spring and winter quarters which were below the average temperature, than in those which were above it. 6. That, as in smallpox, the average highest mortality during seventeen corresponding weeks in 1840—56, was more than double the average lowest mortality. 7. That scarlet fever presents one period of highest and one of lowest rate of death, and this more markedly than either smallpox or measles; the greatest number of deaths occurring at the end of October or beginning of November, the lowest at the middle or end of March, or early in April. 8. That the average mortality is higher in warm springs than in cold. 9. That the mean of the greatest number of deaths in seventeen corresponding weeks of 1840—56 was about twice and four-fifths as large as the mean of the lowest number of deaths in the seventeen corresponding weeks. 10. That there is not any particular short period at which it can be said to reach its culminating point. 11. That the greatest rate of death occurs in autumn, and the smallest in spring, the differ-

ence between them being less than in any other eruptive fever. 12. That the average maximum number of deaths in seventeen corresponding weeks in the years 1840—56, was not much more than one-half that of the minimum. 13. That the period of greatest mortality from diarrhœa is very definitely marked, occupying a period of about seven weeks (from the 31st to the 37th), during which more than two-thirds of the annual mortality takes place; the greatest rate of death extending from the last week of July to the first week in September. 14. That this period corresponds with a mean weekly temperature of 60.5, or above.

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21. *Illustrations of Aguish Disorder prevailing in London during the last two or three Years.*—Dr. C. Handfield Jones, in a communication recently presented to the Royal Med. and Chirurg. Soc., stated that, while he entertained a doubt whether the cases selected presented any very striking points, he believed that the series were not without value as illustrating a frequent character of disease; and he hoped that their consideration might shed some light on various morbid phenomena of common occurrence, yet but imperfectly understood, and often misinterpreted, and so might render their treatment more rational and satisfactory. He wished the cases to be regarded as experiments in which the nature of the morbid action was, to some extent, inferred from the mode in which it was affected by remedial agents. He did not consider the evidence demonstrative; he only claimed for it that the inferences should be regarded as highly probable. Information of this kind, though very imperfect, may, after all, be of far more use than the most exact determinations of the balance and the microscope.

He premised the following statements:—

1. That local heat, congestion, and sometimes inflammation, may be produced by paralysis of the vaso-motor (sympathetic) nerves of a part.

2. That the phenomena of fever can be well accounted for on the view of paralysis of the sympathetic system.

3. That there is good reason to believe that neuralgia and dysentery may be the exponents of malarious influence as much as a true ague or remittent.

4. That there is much cause for believing that the vicinity of canals is unhealthy, and that they generate a poison producing effects like those of malaria.

5. That the evidence respecting the production of malaria is such as makes it impossible to affirm that it may not be generated by any moist surface in the process of drying.

6. That, in cases of obscure and refractory character, the reaction of the system towards remedies, and the effect of change of air, may afford most material aid towards forming a correct idea of the cause and nature of the disease.

7. The term malarious disorder, as used by the author, is not intended to assert that the essential cause is always identical with marsh miasma; but to convey the idea of a disorder, which, however produced, is exceedingly similar to intermittent and remittent fever in many respects, maintaining the same pathological affinities, operating on the same (nervous system), and requiring the same treatment.

8. The primary action of fever poison, most notably of malarious fever, in paralyzing and depressing affections, especially the nervous system. The most deadly fevers are the algide, in which reaction never occurs; the phenomena usually called febrile are, if the excitement be not over great, a desirable indication that the system rallies, and resists the morbid influence. Hence, if, when there is no intense malaria, a patient suffer from continual, exceeding, causeless (apparently), depression and debility, occasional chills and flushes or perspirations, with an interlude of neuralgia at times, or a dysenteric or rheumatic attack, and if benefited materially by quinine, iron, &c., and, most of all, by change to a healthy locality, the inference that he laboured under some aguish disorder seems legitimate. The system is habituated to the morbid action, and does not react strongly against it.

9. The tendency to relapse is most difficult to eradicate. Any cause of a

debilitating kind will almost surely reproduce the morbid action in some form or other.

He then gave the details of the following twenty-one cases:—

1. Obscure aguish disorder.
2. Rheumatic nasal inflammation.
3. Cerebral attack; epistaxis. Benefit from quinine.
4. Cerebral attack; failure of cerebral power. Benefit from ferrocitrate of quinine.
5. Vomiting, febricitation, head-pain, exposure to ague poison. Cure by quinine and strychnia.
6. Cerebral attack; canal influence.
7. Scrofulous, epileptoid seizures. Cure by ferrocitrate of quinine and nux vomica.
8. Hemiplegia in a neuralgic subject. General tonic treatment—recovery.
9. Hemiplegia, first of right, afterwards of left side; canal influence.
10. Rheumatism of the œsophagus; canal influence.
11. Rheumatism, thoracic neuralgia; depression; canal influence.
12. Cardiac neuralgia paralytica; slight hæmatemesis. Cure by quinine and iron.
13. Rheumatoid intrathoracic neuralgia.
14. Severe cardiac neuralgia; aguish attacks.
15. Periodic gastralgia. Cure by quinine and iron.
16. Rheumatic abdominal neuralgia.
17. Febrile affection; lingual neuralgia; cure by quinine and iron.
18. Dysentery and aguish disorder; cure by quinine and opium.
19. Dysentery, rheumatism; Crimean origin.
20. Nervous prostration, hemorrhage from stomach; benefit from change of air, cinchonine, and strychnine.
21. Fugitive œdema recurring frequently in various parts; cure by sustained tonic treatment.

The author then proceeded to cite ten instances in which more decided ague was originated or developed by a residence in London. In conclusion, he drew the following deductions:—

1. There seems much reason to think that an obscure form of aguish disorder prevails in (at least) the western parts of London, the phenomena being chiefly great debility, hyperæsthesia, failure of cerebral power, neuralgia of external or internal parts, gastric disorders, rheumatic affections, and ill-developed febrile paroxysms; quasi-apoplectic seizures are not uncommon.
2. Quinine, iron, and strychnine, are found of decided benefit, with a generous diet.
3. Change to a pure air is the best remedy.

It must be apparent that, if there be any truth in the foregoing views, those who ignore them entirely must practise at great disadvantage, and meet with continual disappointment. The opinions advanced in the paper are mainly derived from Dr. Macculloch's writings, whose opinions are strongly corroborated by Dr. Copland.

Dr. Webster believed that ague was now far less common in London than formerly. The disease was a very common one two or three hundred years ago, James I. and Cromwell having died from it. When the population of London was not one-fourth its present amount, the deaths from ague were more numerous than now. The average number of deaths in London from real ague did not exceed twenty-four or twenty-five per annum. The disease was more common in the western parts of London than in the other districts, a circumstance which he attributed to the excavation of a considerable quantity of virgin soil on which the new houses were erected, and to comparatively inefficient drainage. He agreed with the author in thinking that residence near canals was productive of aguish disorders, and instanced a case in which out of 1400 inhabitants in a village on the Rhine, in the neighbourhood of a new canal, more than 1100 suffered severely from those maladies, accompanied in many instances by dysentery. The disorders were more common in men than in women, and in middle than in early life, though the most severe case

he had seen was that of a child, six weeks old, in the hospital at Tours, a district surrounded by canals and rivers. Quinine was doubtless the best remedy; but he had seen some exceptional cases in which that medicine had had no effect, and which had been successfully treated by purging. He should not be disposed to give quinine with strychnine, thinking that the one would counteract the effect of the other.—*Med. Times and Gaz.*, July 18, 1857.

22. *On Spanæmia, Chlorosis, and Analogous Conditions, as the Predominant Characteristic of the Present Age.*—Dr. POLLITZER takes a very gloomy view of the condition of the human race at the present time, and considers it to be an established fact that the physical deterioration in Europe is profound, "a sad memorial of civilization." He admits the general diminution of mortality in all civilized countries, but affirms this to be a fallacious test, as there is not a corresponding increase in the health and vigour of the race, or in the number and character of the diseases. The reduction of the mortality, the author attributes to the increase of hospitals and similar charitable institutions—to quarantine, vaccination, and numerous sanitary regulations. The boundaries of health and disease, he observes, are daily becoming less marked, and he considers it characteristic of modern pathology to affirm that there are numerous conditions which are undoubted deviations from the healthy standard, though it is impossible to delineate or give definite portraits of them, because they make their appearance during a state of "relative health." The physician has no name for the disease, but the patient maintains that, not feeling in health, he has no alternative but to call himself ill. This anomalous condition, Dr. Pollitzer accounts for by the spanæmia and chlorosis, which he regards as the feature peculiar to our times—the soil in which the feebleness and deterioration of our race take root. After developing his views more in detail, the author proceeds to show how these conditions are fostered by modern civilization. A constant stretch of the mental powers—a restless excitement of the passions—a perpetual struggle for advancement—the fresh wants of every day, science and the arts themselves being subservient even to the luxury and demoralization of the times—the destruction of all moral harmony and peace—are advanced by Dr. Pollitzer as the evils of modern civilization. And these evils react especially upon the younger generation; and the demands made upon the youth of eighteen or twenty of the present, would formerly have been considered a sufficient tax for the strength of a man of upwards of five and twenty. He inveighs especially against the polymathy (if we may coin the word) of children, among whom the spanæmia and chlorosis of the age especially flourish.

Having for seventeen years devoted himself to the study of children's diseases, he has arrived at the conclusion that the features which characterize our age have their source in the treatment of childhood, and that the deterioration of the race at large takes its origin in that of childhood.

The facts upon which Dr. Pollitzer bases his remarks are, that anæmia and chlorosis occur alone, or associated with rickets, hypertrophy of the lymphatic glands, and of the spleen and liver, to an incredible extent, even from the first month of life. Of 1,000 children that were treated in the children's hospital, on an average, 700—800, or from 70—80 per cent., were thus affected. He also observed that the anomalies of the blood and constitution, which are so widely diffused, invariably appear where the nutrition of the child has been imperfectly effected. The stomach and intestinal tract are the parts that first suffer; hence it is in these organs that we discover the prevailing morbid conditions of childhood; and while they materially influence the mortality of children, they equally affect the state of their future health when they survive childhood.—*British and Foreign Medico-Chirurgical Review*, July, 1857, from *Zeitsc. des K. K. Gesellsch. der Aertze*, February, 1857.

23. *Redness of the Cheeks as a Symptom of Pneumonia.*—Dr. A. GUBLER takes up the old doctrine that the redness of a cheek in a case of pneumonia, indicates the side on which the disease lies. Modern authors have paid little attention to the subject, but Dr. Gubler has satisfied himself, by extensive obser-

vation in the Salpétrières, that the general law is true. The author has guarded against the fallacy which might result from the patient lying on the cheek presenting the greater redness, and has measured the relative temperature of the two sides of the face with the thermometer. Numerous cases are detailed, and the following is the summary of his observations: 1. The redness of the cheeks, which commonly coincides with pulmonary inflammation, is not, as is commonly thought, a fortuitous circumstance, but a functional disturbance bearing a definite relation to the disturbance of the respiratory passages. 2. This redness is not necessarily proportioned to the extent and degree of the anatomical lesion, but bears a relation to the intensity and progress of the inflammatory action. 3. A sensible, and sometimes considerable elevation of temperature (from 0.50° to 5.40° Cent., or nearly 1° to 10° F.) accompanies the hyperæmia, and gives it the character of active congestion. 4. The congested cheek corresponds to the lung which is the seat of phlegmasia, or the one which is most affected. 5. The flushed cheek is seen, not only in pneumonia, but also in the majority of other pulmonary inflammations—in those which accompany tuberculization, as in typhoid pneumonia, and even in capillary bronchitis. It appears to be most marked in pneumonia of the apices—a circumstance already pointed out by Bouillaud. 6. The production of other morbid conditions may be promoted by the habitual hyperæmia of the face; thus a spot of erysipelas has been seen developed on the cheek of the affected side. 7. The redness of the cheeks in acute diseases of the lung may be explained by the stimulation of their nervous plexuses extending to the brain, and reflected upon the respiratory nerves of the face. 8. The phenomenon may be regarded as a manifest example of sympathy established between two distant regions by the agency of the nervous system.—*British and Foreign Medico-Chirurgical Review*, July, 1857, from *L'Union Médicale*, April and May, 1857.

24. *New Premonitory Symptom of Cholera.*—Dr. Poznanski, in a communication to the Imperial Academy of Sciences, states:—

1. That during the prevalence of cholera it frequently happens that the pulse is extremely low, and reduced to 45 or even 42 in persons apparently in perfect health. 2. That this symptom is unaccompanied by any other denoting a morbid state. 3. That when the pulse is low, the blood becomes dark and viscous; while in persons whose pulse is in a normal state during the epidemic, the pulse is perfectly healthy. 4. The cholera only attacks those that have previously experienced a diminution in their pulse. 5. That this diminution, which often occurs weeks before the regular attack, may be considered a pathognomonic symptom of the approach of cholera. 6. That those who have experienced the diminution in question have always escaped the disease whenever they have followed a regimen calculated to accelerate the circulation. 7. That the falling off of the pulse, and therefore the predisposition to the disease, are in general proportional to the want of energy in the circulation of the blood and to the excess of atmospheric pressure. 8. That this diminution does not occur in healthy subjects when the epidemic has ceased.—*British Med. Journ.*, Aug. 1, 1857.

25. *On the Application of the Microscope to the Diagnosis of Pulmonary Consumption.*—Dr. Theophilus Thompson recently read before the Harveian Society, an interesting paper on this subject. We observed that the assiduous and discriminating use of the microscope having yielded valuable aid in illustrating various pathological questions, and giving precision to some grounds of diagnosis, it seemed reasonable to anticipate information from the application of this instrument to the examination of expectoration; and he thought it practicable to show that the disappointments which some observers had experienced in their endeavours to avail themselves of this method of investigation did not depend on inadequacy of the means, but might be overcome by care and perseverance. Many years since, Mr. Quekett detected elastic pulmonary tissue in the sputum of patients not previously considered con-

[1] Nosographie Médicale, tom. xi. p. 484.

sumptive, and it was for a time supposed that a peculiar granular appearance of the expectoration might be regarded as characteristic of tubercular disease, even in the absence of any trace of elastic tissue. Finding, after a time, that this appearance could not be relied on as an indication of incipient phthisis, and embarrassed by the multiplicity of objects often present in the expectoration, Dr. Thompson for a time discontinued the investigation, any sanguine expectations which he had entertained being further discountenanced by the testimony of Rainey, Addison, and Bennett;[1] but in an interview with Dr. Andrew Clark (to whose sagacious observation and faithful descriptions[2] the profession is greatly indebted), he had the gratification of learning that the subject had engaged his attention since the year 1846, and with such success as to enable him to show, in his lectures at Haslar, the real microscopical indications of tubercular sputum. With his friend's liberal and courteous assistance, Dr. Thompson soon became convinced that changes in the pulmonary vesicles, preceding the stage of destruction which occasions the elimination of pulmonary tissue, are manifested in the expectoration, and that information may thus be obtained, not only supplying valuable aid in diagnosis, but also furnishing instructive information regarding the morbid process concerned. Dr. Thompson showed, by a diagram enlarged from a drawing by Shrœder Van der Kolk, that when tubercular deposit is present in the pulmonary vesicles, there may be seen, contrasting with the usual epithelial cells, some which are dark, swollen, spherical; some more advanced, larger, and misshaped; others shrivelled or burst, and extruding nuclei, which nuclei, when enlarged, correspond with the "tubercle corpuscles" of Lebert. The author proceeded to show that the sputum of consumptive patients contains materials corresponding in appearance with the elements present in the air-vessels, and that before an amount of disease involving the elimination of elastic areolæ occurs, corpuscles of various sizes, jagged outline, setting free nuclei, and affording evidence of rapid disintegration, may be detected. The general moleculo-granular appearance (to which his attention had been originally directed, and which he much regretted having erroneously figured in his "Clinical Lectures") was not conclusive; the sputum which is really characteristic containing isolated masses of moleculo-granular material, and having interspersed corpuscles of various forms, overgrown or jagged, and setting free nuclei; the various proportions of pus, or fat, or blood, giving collateral indications of the amount of surrounding deterioration in the lungs; while amongst evidences of rapid progress might be specified the appearance of large and numerous areolar meshes, still retaining their adhesion and elasticity. In chronic cases, portions of this tissue appear, inelastic, teased out, and broken down, in consequence of long imprisonment, whilst a diminished proportion of fat, and the appearance of cholesterine plates, and still more of earthy particles, were often indicative of a mode of restoration. The author proceeded to prove, by a brief narration of cases—

First, that with the aid of the microscope positive conclusions, not attainable by auscultation, could sometimes be formed regarding the existence of pulmonary disease.

Case I.—Mr. ——, aged sixty-three, after an attack of pleurisy in the left side, during the spring of 1855, did not regain strength. Dull percussion, and prolonged expiratory murmur over a small portion of the right apex were the only important auscultatory signs; but the expectoration, under the microscope, was found to contain blood corpuscles, moleculo-granular matter, and lung tissue broken down and unbent. More positive symptoms of decided phthisis, as reported by his medical attendant in the country (Dr. Sylvester, of Trowbridge), soon appeared, and in a few months he died.

Case II.—Mrs. E——, a lady, aged thirty-nine, whom Dr. Thompson attended with Mr. Marshall, of Bedford Square, during the early months of 1855 suffered

[1] Dr. Hughes Bennett has lately added his testimony to the value of the microscope in suspected phthisis. Vide Edinburgh Monthly Journal, Jan. 1856, p. 585.

[2] Vide Transactions of the Pathological Society, vol. vi. p. 74; and Lettsomian Lectures, by Theophilus Thompson, M. D., F. R. S.

from obstinate sickness, which was supposed to depend on gastric affection. The persistency of the sickness and the progressive emaciation inducing an examination of the chest, some dulness on percussion with increased vocal thrill was observed near the sternal end of the second intercostal space on the right side. A little expectoration was obtained, and was found to contain shrivelled cells, lung tissue, and isolated masses of granules. Some improvement of the general health occurred under soothing hygienic and tonic treatment, and the administration of cocoblein. But early in the year 1856 the expectoration became copious and flocculent; dulness on percussion was more extensively obvious; near the inferior angle of the scapula click was audible, shortly followed by cavernous breathing. In March she died. An interesting contrast to this history was afforded by

Case III.—A lady, aged thirty-eight, who, in the autumn of 1852, had almost precisely the same auscultatory symptoms as were observable in Mrs. E——; but the occasional, slight, cloudy expectoration, from time to time examined, exhibited ciliary cells, some with long tails, probably tracheal, some in masses, as though from the follicles; but there were no tubercular elements. In harmony with the encouraging testimony thus afforded by the microscope, the general symptoms continue favourable, and have hitherto, during a period of five years, negatived the gloomy prognostications which an accomplished auscultator had perseveringly maintained.

Secondly: the author adduced the advantage of microscopical observation in confirming doubtful signs.

Case IV.—E. T——, aged fifty-one, in the winter of 1854 was attacked with cough, hurried breathing, and some symptoms of hectic. The left lung had been extensively consolidated in consequence of pleuro-pneumonia ten years previously. Over a small space near the lower angle of the left scapula a sound could be heard, of which it was difficult to determine whether the correct designation were subcrepitation or click. Dr. Andrew Clark, who also obligingly examined the expectoration, reported that it contained shrivelled cells, large cells with shrivelled nuclei, and some earthy matter, and, without receiving any history of the case, offered the diagnosis of "Slight tubercular deposit, tending to restoration;" a diagnosis which was confirmed by the result.

Thirdly: Dr. Thompson described some favourable indications afforded by the microscope concurrently with amelioration in the general condition.

Case V.—Mr. ——, aged twenty-two (introduced by Mr. Pinching of Gravesend), five feet nine inches in height, in the summer of 1854 had dull percussion and a murmur over the left pulmonary artery, but no crackle or click; the expectoration, however, exhibited lung tissue, tubercle corpuscles, and blood-disks. He took cod-liver oil freely, at one period to the extent of a pint and a half in a week; and had ioduretted neat's-foot oil (a grain to the ounce) rubbed into the chest. After a time the expectoration became chiefly bronchial, disposed to fibrillate, and free from lung tissue. The weight of this patient increased from ten stone one pound to eleven stone nine pounds. He spent last winter in Madeira.

Fourthly: the author noticed the important evidence sometimes derivable from the sputum, indicative of rapidity in the progress of disease.

Case VI.—A lady in the country, aged forty-three, who had been for two years the subject of phthisis, but whose friends did not fully realize the danger, had a decided aggravation of cough and weakness. Some expectoration, sent to town for examination, contained blood, copious pus corpuscles, and numerous large meshes of pulmonary tissue, perfectly retaining their form and elasticity. A very unfavourable prognosis was consequently given, which was verified by the death of the patient a few days afterwards.

The author, in conclusion, ventured to express the opinion that his statements, although brief, were sufficient to support his proposition, that the microscopical inspection of expectoration might often afford, at a very early period of consumption, definite information, not otherwise attainable, regarding the nature of the malady; that in later stages of disease it might assist us to estimate the rapidity and progress, and at all times might furnish valuable aid in forming a correct prognosis regarding the course of the complaint. He

trusted these few suggestions would stimulate to the investigation some of his professional brethren more accomplished in the use of the microscope, or more fortunate in the enjoyment of leisure.—*Lancet*, July 11, 1857.

26. *On Movable Kidneys.* By Prof. Oppolzer.—The knowledge of the possibility of the existence of this affection is of importance to the practitioner, as, when unaware of it, he may suppose the appearances observed to result from various other causes, and submit the patient to an injurious course of treatment, or give rise to unnecessary alarm upon his part. The abnormal movability usually affects but one kidney, and especially the right one; but the author has met with cases in which it was observable in both, and that in a remarkable degree. In all the cases he had the opportunity of examining, the patients dying of some other disease, the kidneys were found healthy; but in these cases there has been observable a deficiency in the cushion of fat, and an extension of the renal vessels. In some cases, the practitioner's attention has been drawn to the abnormality by the patient observing a tumour on one or both sides of the abdomen, which only became perceptible while standing, or lying on one or the other side, disappearing again during the horizontal posture. Generally speaking, however, it is first discovered by careful exploration, when, beneath the relaxed, painless, and not very obese abdominal parietes, a largish, rounded tumour is perceived deep under the liver or stomach. The inner concave side can only be felt in very thin persons, and the upper end is only accessible in some. The tumour can easily be pushed upwards, and then may suddenly disappear, but it cannot be pressed against the spinal column, or downwards below the crista ilii, without great pain being produced. To very firm pressure, made in any direction, the tumour is sensible; and the patient spontaneously complains of a sense of pressure and dragging, especially when standing, performing active movements, during defecation, etc. In the cases seen by the author, the condition of the urine has been normal.

The affection is usually congenital, as is shown by the lengthened condition of the vessels. Rapid emaciation occurring in persons formerly fat, concussion of the body, as in rough travelling, constipation, etc., may probably contribute to its production. In fat persons, the diagnosis may be impossible, but it is not difficult in those who are thin, as the form of the swelling can be traced, while the tumour can be pushed into the lumbar region, and there felt. The pain which it not unfrequently gives rise to cannot be mistaken for neuralgia, colic, or rheumatism, if the practitioner will only make an exact exploration; while the tumour resulting from a collection of feces assumes another form, and does not appear in, or disappear from the lumbar region in consequence of pressure. It may also be distinguished from a movable spleen, as the latter lies in front of the intestines under the parietes, and gives rise to dulness on percussion, which the kidney does not. It can only be confounded with cancerous and tubercular masses, when these are movable, and resemble the kidney in form. Treatment of this affection by bandages, and the like, is of no avail; and the removal of pain when present must chiefly be sought from the horizontal posture. Confinement of the bowels, and the consequent straining, must be avoided. It is, however, of great importance to be able to tranquillize the mind of the patient as to the nature of the affection, and to prevent injurious measures being adopted; and hence the value of a correct diagnosis.—*Med. Times and Gaz.*, June 6, 1857, from *Wein Wochenschrift*, No. xlii., 1856.

27. *Open Foramen Ovale in the Adult.*—Some years since (1851), Dr. J. W. Ogle examined sixty-two human hearts, with a view of discovering the condition of the foramen ovale, and found that of these there were thirteen in which this foramen was incompletely closed, or one in five. The patency noticed allowed of very different degrees of communication, between the two sides of the heart. "In some of the cases, the opening remaining in the septum of the auricles was a mere fissure or oblique slit, arising simply from want of such an adhesion between the valves and the margins of the isthmus as is wont to

exist after the completion of the drawing up of the valvular fold and other preliminary contractile actions of neighbouring parts. This defective adhesion, I may remark, is thought by Dr. Peacock to be the result of unequal pressure in the auricles from the blood, owing to the slow establishment of the pulmonary circulation after birth. The oblique slits, to which I have alluded, were directed variously, as it were, opening in some cases more widely from above, and in others from below, and would have allowed of the blood passing, as it would seem, more freely in some cases from the left into the right auricle, but in the majority of cases from the right into the left auricle. In several of these cases, the communicating opening was not merely that of a chink or slit, but was a decided round or oval aperture; in one case admitting of the entrance of the tip of the little finger. In one or two cases the opening was of a reticular character, having the appearance as if it had been produced by a falling short of one part of the margin of the valvular fold, so that it failed to be raised to the level of the isthmus, to which it ought to have been adherent; and as if the deficiency had been in part compensated for by bands passing across the vacancy. I was careful in my search for any trace of ulceration or of rupture which might have accounted for the opening, but this did not in any case exist."

Dr. O. has recently referred to the life histories of these cases, in order to ascertain what physical signs were noticed before death, which might be attributed to this imperfect closure: and he states that in these thirteen cases "there are seven in whose histories it is emphatically stated or to be inferred that no murmur synchronous with the systole of the ventricles, that is also synchronous with the diastole of the auricles, existed; and only one in which there is notice of any murmur with the diastole of the ventricles—that is, with the systole of the auricles. Consequently, excepting in this last single instance, we have the fact of six hearts in which more or less patency of the foramen ovale existed, but did not give origin to any unnatural sound whatever about the heart. In the solitary instance (Case 3) in which a regurgitant or diastolic murmur (so called) was heard, the heart was very large and soft, and recent granulations and shreds of fibrinous material, deposited from the blood, were adherent to the aortic valve-flaps. It may therefore well be believed that this murmur was seated in the left ventricle, and had nothing to do with the septum of the auricles; if it had any such connection, it must have been produced during the systole of the auricles, and therefore not at all similar to that in Dr. Markham's case, which took place during the ventricular systole, and was supposed by him to be produced during the diastole of the auricles. As regards the last six cases, in which the closure of the foramen ovale was imperfect, I am sorry to say that the life history does not afford data from which any conclusive evidence as to the presence of cardiac bruits may be derived. It will be remembered that in all these thirteen cases the patients were adults. In none of them was there any cyanosis noticed during life, or congenital malformation of the heart or its vessels found after death. Moreover, there was no such condition of the valves or orifices of the heart as should primarily or secondarily tend in any material degree to impede the blood's exit from the auricles, and thus, as it were, throw greater pressure and stress upon the septum along with other parts of their walls, and lead to a mixture of the venous and arterial blood through the foramen ovale. There was, however, in many cases, marked and extensive disease of the lungs, such as their occupation by the products of inflammation or scrofulous deposit, which, though to a less extent, would have a similar tendency as regards the right auricle.

"I may be permitted here, as I have his sanction, to append a case lately brought before the notice of the Pathological Society by Dr. Ogier Ward, which I will curtail. It bears strongly upon the point I am attempting to illustrate.

"The case was that of a child who died eleven days after birth, having been cyanotic and affected with dysphagia and choking on swallowing. The lungs and heart were auscultated during life, and it was noticed that the heart's sounds were natural, no bruit existing. After death, congenital communication between the trachea and œsophagus was found, with enlargement of the pulmonary artery and patency of both the ductus arteriosus and foramen ovale. The

preparation is now in the museum of the College of Surgeons. This case, added to those related above, makes the seventh case in which the foramen ovale was more or less patent during life, and in which we have evidence that no bruit was thereby produced."

In the number of the *British Medical Journal* for April 4, 1857, Mr. Markham relates a case in which a loud murmur, coincident with the systole of the ventricles, was heard along the base of the heart, and in the entire left subclavian region.

"In this case," remarks Dr. Ogle, "nothing unnatural was found after death in the condition of the heart beyond a patent state of the foramen ovale, allowing the blood to pass readily from the right to the left auricle, but not permitting it to pass in the opposite direction, excepting through two narrow slits. Among other remarks in connection with it, Dr. Markham suggests the very important query whether this peculiar state of the foramen was sufficient to account for the very marked and widely extended systolic murmur heard during life; so marked, indeed, that before death it was described as loud, rough, and prolonged, audible over the whole præcordial region, and over the upper part and along the right border of the sternum, and in the whole of the upper half of the interscapular space. Dr. Markham supposes that in many cases of open foramen ovale, under certain circumstances, this state induces murmur during the systole of the heart. If this state of the foramen be accompanied by other unnatural states of the heart producing any murmur, then the murmur caused at the septum of the auricles becomes masked and hidden; and if there be no such disease in other parts of the heart, then no heart symptoms arise, and thus the heart remains unexamined during life, and consequently the aforesaid auricular murmur not detected. He says: 'Is it not possible that such a murmur may have been present in many cases simply of open foramen ovale, but was not discovered during life, because the disturbance to the circulation produced by the same lesion was not of itself of a nature marked enough to excite attention?' Again: 'Cases where the foramen ovale must have been partially open during life, and yet where no symptoms to mark the fact had been observed, are frequently met with; and assuredly in the majority of these cases it would be a complete begging the question to affirm that no murmur really existed during life. It is more correct to say that in the very great majority of cases of simple open foramen ovale disturbances in the circulation do not occur, and therefore no examination of the heart is made during life. Practically speaking, how many cases are there on record where simple foramen ovale has been found after death, in which the observer can affirm that no murmur existed during life? I suspect very few.' It is with this last question only of Dr. Markham's that I am now concerned; and though in what I have brought forward I do not say that I have shown reason for supposing that a murmur cannot, under any circumstances, be produced by the passage of blood through an open foramen ovale, yet I have, as I think, shown that there are numbers of cases, under ordinary circumstances, in which, although facility for the passage of blood from one auricle to another exists, yet no unnatural sound is thereby of necessity produced. This is so, judging from the cases adduced, even when considerable obstruction to the blood's passage through the lungs must in many cases have been present. It may be that with a certain degree of patency, a certain tension of the boundaries of the foramen, a certain quality of the blood as to consistence, and a determinate force with which the blood is propelled by one or other auricle—under all these conditions it may be a murmur might possibly be caused at an open foramen ovale. More extended observations than have hitherto been made on the matter are, however, yet required to establish the fact."—*British Medical Journal*, June 13, 1857.

28. *Combined Constitutional and Local Treatment of Hooping-Cough.*—Dr. R. Pearce states (*Lancet*, April 11, 1857) that in 75 cases of hooping-cough (32 boys and 43 girls), varying in age from 2 to 8 years, which came under his charge during last autumn, in a school containing over a thousand children, he used the local treatment recommended by Dr. Eben Watson, viz., sponging the glottis once a day with a strong solution of nitrate of silver (one scruple

to one ounce of distilled water) by means of a curved probang; and, in combination with this, he ordered Dr. Gibb's nitric acid mixture (dilute nitric acid, twelve drachms; compound tincture of cardamoms, three drachms; water, one ounce; simple syrup, three ounces and a half), a teaspoonful every three hours. He also, from the commencement, gave a teaspoonful of cod-liver oil twice a day, and, at the same time, kept the patients on generous diet and in warm yet well-ventilated rooms. This treatment, he asserts, has in his hands been invariably successful.

29. *On Bloodletting in Pneumonia.* By Prof. WUNDERLICH.—In the course of five years, there have been treated at the Leipsic Klinik 204 cases of pneumonia, of which number 36 (17.06 per cent.) ended fatally; but if we abstract from these those cases which were brought to the hospital *in extremis*, and count only those which were actually treated there, there were then 190 cases with 11 deaths (11.57 per cent.). Among the fatal cases, 3 were treated by bleeding, as were 44 of the cases that recovered, making the mortality of these so treated 6.38 per cent. These fatal cases were examples of pneumonia complicated with the disease of other organs.

In 114 of the patients, loss of blood occurred during the course of the pneumonia, whether from local or general bleeding, epistaxis or menstruation; and of this number 9 (including the 3 treated by bleeding) died, *i. e.*, 7.89 per cent. In 76 cases, no loss of blood whatever occurred during the progress of the case, and of these 13, or 17.10 per cent. died, not including persons brought in agony, and who had not in general been treated by bleeding. Thus it results that—1. In cases in which there was loss of blood in general the mortality was 7.89 per cent. 2. In those in which venesection had been employed 6 38 per cent. 3. In those in which a complete conservation of blood took place a mortality of 17.10 per cent.

The author enters into an elaborate comparative statement of the influence which the loss of blood exerts upon the time and mode of termination of the fever and of the commencement of the healing process. Pneumonia, he observes, possesses, in the vast majority of cases, the peculiarity of commencing with very determinate symptoms (severe chills, unequal distribution of the blood, and rapid increase of the objective temperature of the trunk), which are immediately followed by acute continued fever (increase of temperature, rapidity of pulse, etc.). In favourable cases, there is this further peculiarity, that at about the period of the completion of the exudative process (cessation of increased dulness on percussion, and of the bloody sputa) the febrile symptoms rapidly disappear, the delirium alone continuing awhile if it has been very violent. In this respect, pneumonia approaches the eruptive fevers, and forms a contrast to other inflammatory diseases, as abdominal typhus, pleurisy, peritonitis, meningitis, bronchitis, etc. Wishing to avoid the ambiguity which would ensue upon the adoption of the word crisis, the author designates this passage of the economy from a feverish to a feverless state, *defervescence.* It is no accidental occurrence, but a process which is sometimes rapid, sometimes slow, and may be complete or incomplete, protracted, uninterrupted, or remittent. A rapid defervescence is decisive for the quick convalescence of the patient; but, while cases in which it is remittent are rare, yet, when it is protracted or interrupted, it is of bad augury for the patient, even when the disease is slight.

As a standard for judging the effects of therapeutical agents upon the period of defervescence, the Professor first selects 32 cases treated by expectation, and in which the exact time of its commencement was noted. Taking 10 of severe and 10 of the medium cases, the defervescence commenced at the seventh or eighth day; but, taking the entire number, in adding 12 slight cases, it occurred at the sixth or seventh day. Judging from 9 cases which came under his notice (2 of menstruation and 7 of epistaxis), spontaneous bleeding proved rather favourable, as the improvement dated from the appearance of the bleeding.

Local without general bleeding was followed by recovery in 36 cases. In 26 it was employed either alone or in conjunction with medicines, such as digi-

talis or ipecacuanha, which exert no appreciable effect in expediting the period of defervescence, and in 10 it was combined with tartar emetic, which does exert an effect of this kind. Of the first series, rapid defervescence took place in 7 slight and medium cases in from the third to the sixth day, and in 19 bad cases it varied from the second to the ninth day. In the 10 cases of the second series, it took place from the third to the seventh day.

In 39 cases in which the commencement of the disease could be accurately ascertained, venesection was employed. First, in 18 of these it was employed on the first or second day. In 10 of these there was immediate arrest of the process; in 2, immediate arrest with a somewhat slower continuance of improvement; in 5, a considerable diminution of fever, with a later but less considerable return, the fever ceasing in 4 cases on the sixth, and in 1 on the seventh day. In 1, no effect was produced, improvement following only after local bleeding. Secondly, in 21 the venesection was performed from the third to the fifth day; but in none of these cases was bleeding the only means employed. The results obtained even here contrasted very favourably with those obtained by expectative treatment. It was found that the conjunction of tartar emetic hastened the period of defervescence somewhat; that of local bleeding was scarcely of any effect, while the addition of digitalis was of no effect whatever.—*Med. Times and Gaz.*, June 6, 1857, from *Virchow's Archiv.*, 1856.

30. *Glycerine in Consumption.*—There is much difference of opinion among medical men as to the influence of glycerine upon cases of consumption, some considering it scarcely inferior to cod-liver oil, while others regard it as valueless. With a view of testing its power, Dr. R. P. Cotton administered it in doses of one, two, and three drachms, twice a day, to twenty-three of the in-patients of the Consumption Hospital, Brompton. In only five of these was there any improvement, in all of which the weight was slightly increased. In two of these, however, a much greater advantage was subsequently gained under the use of cod-liver oil, the weight of one patient having increased as much as two pounds per week.

In seventeen cases, either there was no appreciable improvement, or the patients became worse; and one, in an advanced stage of the disease, ended fatally. In nine of these cases, more or less improvement occurred from the after use of the oleum aselli; in four instances, indeed the gain in weight was very distinctly marked.

In five cases, the glycerine either caused sickness, or otherwise disagreed with the stomach.

To any objection which may be raised that the glycerine was not given for a sufficiently long period, I would merely observe that, even in the cases where some improvement was noticeable, it appeared to me so probable that far greater good would accrue from the cod-liver oil, that I regarded a further trial of the other as unjustifiable; and that such an anticipation was, in some instances, at least, not ill founded, the table sufficiently demonstrates.

The following conclusions are, Dr. C. thinks, irresistible, viz.:—

1. That glycerine has generally but little influence upon phthisical cases.

2. That, as a remedial agent in consumption, it will bear no comparison with cod-liver oil.—*Med. Times and Gaz.*, June 27, 1857.

31. *Electricity in the Suppression of the Lacteal Secretion.*—M. Becquerel, in a late communication to the Société Médicale des Hôpitaux de Paris, has made some remarks upon the influence of electricity in restoring the secretion of milk. His attention was called to the subject by a case related to him by M. Aubert, who had employed electricity in the case of a young woman whose milk had been suppressed in consequence of a double pneumonia. The electricity was applied to the breasts by means of moist excitors, and after four applications, each lasting twenty minutes, the lacteal secretion was completely restored. M. Becquerel was at first incredulous as to the reality of the result; but the following case, which fell under his observation, removed his doubts:—

A young woman, aged twenty-seven, well formed, although of a nervous temperament, had suckled a young infant for six months, but, on the occasion of

some intense and often-repeated mental emotions, the lacteal secretion diminished considerably; the right breast retained a little milk, but the left was almost completely dried up. M. Becquerel applied the electrical current at first to the left breast, placing the moist excitors, made of sponge, successively in the different points of the circumference of the breast, so that the currents might traverse the organ in all directions. Three applications were made, each lasting a quarter of an hour. The patient suffered very little, and indeed experienced little more than a feeling of inconvenience. From the time of the first application, the rush of milk supervened almost immediately after the application of the electrical currents. After the third application, the secretion was full and entire; the child had taken the breast, and the milk was abundant in the left breast, and sufficient in the right to obviate the necessity of applying the electricity on that side.—*Dublin Hospital Gazette*, July 10, from *L'Union Médicale*, Jan. 3, 1857.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

32. *Hereditary Transmission of Tendencies to Cancerous and other Tumours.*—Dr. James Paget has made some interesting statistical investigations concerning the hereditary transmission of cancerous and other diseases.

Of 411 patients which he has tabulated, 244 had cancerous, *alias*, malignant tumours; 147 had non-cancerous, *alias* innocent tumours; and 10 had non-cancerous, but recurring tumours.

Among the 254 with cancers, 60, or 23.6 per cent. had relatives of the same or former generations with cancerous or other tumours.

Among the 147 with tumours neither cancerous nor recurring, 27, or 18.3 per cent. had such relatives with cancerous or other tumours.

Among the ten with recurring tumours, 4, or 40 per cent. had relatives with cancers.

According to these numbers, the respective hereditary tendencies to cancerous and to non-cancerous tumours would seem not very different. But an examination of the second and third tables discovers a great contrast between them.

Among the 60 cancerous patients whose relatives had tumours, not less than 57 had cancerous relatives; but among the 27 patients with tumours neither cancerous nor recurring, there were only 12 who had relatives with tumours like their own. The comparison of the respective probabilities of hereditary transmission would, therefore, be as 22.4 for the cancerous to 8.2 for the non-cancerous.

The contrast appears the stronger in sight of the fact that the greater part of the instances in which similar innocent tumours occurred in more than one member of the same family were limited to one kind of tumour. Of the 12 patients who had relatives with innocent tumours like their own, 7 had some form of cutaneous cyst of the scalp, leaving only 5, or 3.4 per cent., as marking the probability of hereditary transmission in all the other forms of innocent tumours reckoned together.

It is hence certain that cancerous disease, or a tendency to it, is prone to pass by inheritance from parent to offspring, and to occur (probably by inheritance of common properties) in many members of the same family and generation. It may seem unnecessary to bring evidence of a fact so generally believed; but there are some who doubt it, and many who are not aware of the large proportion of cases in which cancer may be referred to an hereditary origin. Moreover, a comparison shows, by the contrast of the two groups of cases, the cancerous and the non-cancerous, how many instances of apparently hereditary origin of the disease may be referred to accidental coincidence, or to the transmission, not of a diathesis, but of some peculiarity of the structure

or composition of a part. When, for example, the child of a cancerous parent has a sebaceous cyst in the scalp, we can only count it as an accidental coincidence; but the cases of this kind are not very uncommon; and it might be right to endeavour to estimate from them a deduction to be made in the reckoning of the proportion of actually inherited cancers. For if, thus, dissimilar tumours may accidentally occur in members of the same family, so, in a certain number of cases, the occurrence of similar tumours in the same family may be referred to accident. However, as the deduction to be made on this ground can scarcely be calculated, and would certainly be less than the addition that we should have to make if we could reckon the cases of inheritance from patients with unknown internal cancers, I omit it, and thus sum up the general conclusions to be drawn from the tables.

	Per cent.
Of patients with non-cancerous tumours, the proportion that has, or has had, relatives with tumours like their own, is	8.2
Do. with tumours non-cancerous, but unlike their own . . .	4.
Do. with cancerous tumours	6.8
Of patients with cancerous tumours the proportion that has, or has had, relation with cancers, is[1]	22.4
Do. with non-cancerous tumours, is	1.
Of patients with recurring non-cancerous tumours the proportion that has, or has had, relatives with cancers, is	40.

A few words may be added concerning the last-named cases. The number of instances of these recurring tumours, which occur in the members of cancerous families, justifies an opinion which I have long entertained, but which it is very difficult to establish, namely, that such tumours often represent what may be called a gradual fading of the cancerous diathesis. The cases which I have tabulated are only those which I have seen; but I have heard and read of others like them, and believe that time will prove that, among the offspring of cancerous patients, and among the members of families in which cancer has occurred, there is a peculiar liability to the production of tumours, which will recur after repeated and complete excisions, though they are neither cancerous in structure, nor attended with similar disease in the lymphatics or other organs, nor with any cachexia but such as may be ascribed to their gradual influence on the constitution.

If this can be proved, we may justly hold that this character of recurrence indicates the existence of the cancerous diathesis, either with less abundance or with less concentration of material than is required for the production of a cancerous tumour with all the typical properties.

The imperfection of the diathesis may in some cases indicate its progressive, but as yet incomplete, development; but the probability that, in other cases, the diathesis is decreasing in hereditary transmission, is supported by facts in the history of other diseases. Thus, we know many of the liabilities of the offspring of gouty, and of phthisical, and of insane persons, and that their diseases, though they may be neither gout, nor phthisis, nor insanity, are yet of the same kinds, essentially the same diseases, but less manifested; or of the same type, but less strongly marked; and we know that success in the management of these diseases depends, in great measure, on a due recognition of their parentage. Now, the recurring tumours, I believe, illustrate the same principle in the pathology of cancers; and if so, they deserve the closest study, as being palpable examples that, in the hereditary transmission of morbid conditions, there is a tendency towards their becoming less, a tendency towards

[1] This is a larger proportion than is stated in my lectures. The difference is probably due to my having here reckoned scarcely any cases besides those of patients whom I have seen, while in the Lectures I referred to cases collected from various sources. I have, probably, inquired with more than ordinary care into the family histories of patients, but I have not questioned cancerous patients more closely or more generally upon this point than I have those with other tumours. On the other hand, I have not reckoned as patients having no relatives with tumours those in whose cases I have recorded nothing concerning their families.

health.[1] This may be due either to dilution, or to changes comparable with those which restore an individual from disease to health; and very probably it is an example of that general law, according to which the deviations from the true specific form and composition, which constitute varieties in species, become in successive generations gradually less, till the perfect specific characters are regained.

In practice, the recognition of recurring tumours suggests caution in speaking of what may follow the removal of any tumours from persons of cancerous family. And this caution should be the greater, the more the removed tumours deviate from the ordinary characters of innocent growths or of the fully developed natural structures. I think that the non-cancerous tumours most likely to recur are those which, at whatever date of growth, have structures similar to those of the natural parts in their very early embryonic state. Such are recurring fibroid, composed almost wholly of elongated fibro-cells, and the recurring fibro-cellular, cartilaginous, and mammary-glandular, in all of which we find abundant soft or liquid transparent blastema, in which the proper structures, when they are to be found at all, lie loose and disorderly in their most immature forms. Indeed, whether there be any suspicion of cancerous inheritance or not, all such soft tumours with imperfect embryo structures may be regarded with fear of their recurrence after excision.—*Med. Times and Gaz.*, Aug 22, 1857.

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33. *Fracture of the Thigh for the fourth time at the same part.*—Sarah Hales, aged 60, a stout, rather pale, but not unhealthy woman, in 1847 caught her foot in the carpet and fell, breaking her left thigh-bone a little above the knee. In seven weeks she was allowed to get up, and in twelve weeks could walk again. In 1850, her foot slipped on a potato, and she again fell, breaking the bone at the same part. In six weeks it was united, and in eleven weeks she was able to get about. In 1852, when raising herself from the floor, her hand slipped, and falling upon that knee the bone was broken at the same place for the third time. She was able to walk in about the same time as before. In January, 1856, when walking along the road her foot slipped and she fell, the thigh-bone giving way for the fourth time at the same part. Whether the bone gave way before she fell, or whether it was broken by the fall she could not tell. She had not much pain, but knew the limb was broken because she had lost the use of it. She was brought to Cambridge the next day; and, when examined, the fractured portions of the bone, which were thickened a little, could be rubbed together as though the ends were rounded and smooth. This gave scarcely any pain, and there was very little swelling or bruising. Mr. Humphry was apprehensive, from this condition of the part, that union would not take place. To give it every chance, the limb was carefully splinted, and perfect rest secured by gum-chalk bandages, etc., which were kept on for eighteen weeks, when, firm union having taken place, she was allowed to get up and go on crutches. These have been some time laid aside, and she can walk without them. The bone is firmly united. There is a little overlapping, but not much thickening.—*Brit. Med. Journ.*, June 6, 1857.

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34. *Spontaneous Fracture of both Thigh-Bones.*—Sophia Low, aged 56, was admitted into Addenbrooke's Hospital, Cambridge, under the care of Mr. G. M. Humphry, April 23, 1857. The following is taken chiefly from her own account: She was a healthy-looking person. She said that her right thigh was broken May 5, 1855, under the following peculiar circumstances. For some years her leg had been subject to pain, chiefly about the middle of the thigh, but extending upwards and downwards from that point. A month before it broke, the pain became more severe, and she raised her leg from the ground with difficulty. Two days before it broke she caught her toe when going up stairs: this caused sharp pain at the middle of the thigh, and a lump formed there. She was walking out when it seemed as though the left foot caught,

[1] I think, too, that the histories of rodent ulcers, and of lupus, should be studied with a similar suspicion of their relationship to cancers.

and she fell; but she is sure that she felt the right leg break *before* she reached the ground. The fracture was treated by a surgeon with splints, etc. It was united in five weeks, so that the splints were removed and the limb gradually got strong again. Since then she had had no more pain in the thigh; and she was well till soon after last Christmas, when a pain, similar to that she had before experienced in the right thigh, affected the other limb. It got gradually worse. In the last two or three days before the thigh-bone broke the pain was very acute, and the limb was in much the same state as the other had previously been before the fracture of it; so much so that she feared it would break. It did so on March 1 in the same way as the other had done. She "kind of made a stumble, and felt it break before she reached the ground." Long splints, etc., were applied in the hospital. The fracture united in the usual time, and is now quite firm. There is no pain now in the thigh; but she is complaining of pain in both her arms, a little below the shoulders.—*Ibid.*

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35. *Fracture of the Humerus from Muscular Action.*—Fracture of the shaft of a bone by muscular action in a healthy person is rare, and its occurrence has even been doubted. Mr. Henry Smith records (*Med. Times and Gaz.*, July 25, 1857) the following interesting example of it:—

He was called to a gentleman 20 years of age who had received some serious injury of the arm twenty hours before. There was immense swelling about the shoulder, but no distortion or shortening of the limb. The patient was so exquisitely sensitive to pain, that it was impossible to ascertain the exact nature of the injury; he was, therefore, placed under the influence of chloroform. Notwithstanding the amount of swelling, it was pretty clear that the head of the humerus was in the glenoid cavity, and, on careful examination, it was discovered that the shaft of the bone was broken at the surgical neck.

It appears that the patient, while walking by the side of the river Thames, took up a stone weighing about two ounces, and endeavoured to throw it across the river; he used an extraordinary amount of exertion, and just as he had his arm extended and was about to deliver the stone, the limb dropped and he became faint. He was soon seen by two medical men, who were somewhat puzzled as to the exact nature of the injury. One of them thought that the head of the humerus was dislocated, and the other was of opinion that a fracture had taken place, but they could not quite determine which had occurred.

On making further inquiry, I ascertained that this gentleman was in perfect health, was endowed with remarkable muscular power and agility, displayed in wrestling and rowing; had never shown any weakness in his osseous system; had never had syphilis, or suffered from pains in his limbs.

The limb was put up in the ordinary manner, and a good union has occurred.

Mr. S. also notices two somewhat analogous cases. In the first, a man about 25, the son of a master bricklayer, came to me with a fracture of the middle of the shaft of the humerus, which happened while he was endeavouring to lift himself from the ground by grasping with his hand the top of a wall. Now, this patient told me that for some time previously he had suffered from what are termed rheumatic pains in his arm.

In the other case, the patient was a healthy gentleman, who had his right humerus broken in the following way: He was attempting to force down the arm of another man, their hands being clasped and their elbows resting on the table. My patient, feeling that he was getting the better of his antagonist, made an extra effort, and in so doing his arm broke just above the elbow. Unfortunately the nature of the accident was mistaken, and three persons were pulling him about for half an hour before I saw him, to reduce a supposed dislocation of the elbow-joint. This patient was a perfectly healthy man of great muscular power. In both cases speedy and perfect bony union took place.

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36. *Excision of the Head of the Thigh-bone.*—Mr. Alex. Ure recently read a paper on this subject before the Harveian Society. He considers that morbus

coxarius commences, in the majority of instances, in the head of the thigh-bone; that its origin is inflammatory, and may be usually traced to a mechanical injury of the great trochanter; and that it occurs chiefly in young individuals whose system is under the influence of a scrofulous or tubercular taint. In those cases of the disease in which there is no prospect of a natural cure, and the constitutional powers of the patient are rapidly yielding to the effects of the disease—when successive collections of pus are forming in and around the joint—where the eroded articular cartilage causes acute pain on the slightest movement of the limb, and the patient lies in a helpless state, with the thigh bent on the pelvis and the leg bent on the thigh; in such cases Mr. Ure conceives that the surgeon is justified in removing the head and neck of the thigh-bone. In so doing, he merely imitates what nature occasionally accomplishes by the so-called process of interstitial absorption. It is true that the cotyloid cavity is often extensively diseased as well as the head of the femur, but most frequently the erosion is confined to the upper and back part of the cavity, and may be readily gouged out. Mr. Ure related a case in illustration of his remarks; it was that of a boy, aged 5 years, of a puny and emaciated appearance, who had been suffering from hip disease for the three preceding years. Over the right trochanter there was an unhealthy-looking ulcer of the size of a florin, which was continuous with a sinuous cavity of considerable magnitude, discharging blood with pus upon slight pressure. On examining the rectum, a round and smooth opening was detected communicating with the sinuous cavity just described. All other measures having failed, Mr. Ure determined to resort to excision of the head of the thigh-bone. He made a curvilinear incision about three and a half inches in length, skirting along the upper and posterior border of the great trochanter, keeping clear of the sciatic nerve. He then divided the soft parts over the head of the femur, which was found unattached to the cotyloid cavity, the capsular and the round ligament having been destroyed. Having isolated the head and neck of the bone, he cut through the latter at its base with bone-pliers, and withdrew the excised portion without difficulty. He finally gouged out some carious bone from the side of the trochanter, and also from the upper and back part of the rim of the acetabulum. The portion of bone removed was of a dark red colour, soft, porous, and thoroughly carious; the incrusting cartilage was, in a great measure, destroyed. The wound was closed by suture, and the thigh was kept slightly bent and abducted. In little more than six weeks after the operation, he could move about the ward on crutches, and bend the thigh with the pelvis fixed; the amount of shortening of the limb was but trifling. In the act of progression, he just put the toes and then the heel to the ground. There were two small fistulous openings, from which a small quantity of matter issued in the course of a day. He had gained flesh and strength, his countenance had become cheerful, and his complexion greatly improved. The amendment which had followed the operation was, on the whole, very remarkable.—*Med. Times and Gaz.*, Aug. 8, 1857.

37. *Mr. Fergusson's Case of Excision of an Enlarged Third Lobe of the Prostate.*—This case, an account of which was given in our preceding number, p. 257, we learn from a late No. (May 23) of the *Medical Times and Gazette*, terminated fatally. The operation was performed on the 11th April. Ten days after the patient sank into a feeble state, and continued gradually to decline until the 30th, when death took place. The autopsy showed the bladder congested, hypertrophied, and much inflamed. Its mucous membrane was coated by thick flakes of adherent false membrane. The wound was wanting in action, but not otherwise unhealthy. It would, of course, be unauthorized to infer that the bold practice adopted had any material influence on the result. According to the facts furnished by the Statistical Reports of operations in the London hospitals during the last four years, at least half of the lithotomies in adults end fatally. Whether in a case of greatly enlarged third lobe of the prostate the impediment thus offered to the complete evacuation of the bladder, in case it were left, or the increased size of the wound made were it removed, would be likely to exercise the more prejudicial influence on the result, will probably be a question upon which opinions will not differ. Granting, how-

ever, that the immediate danger of an operation would be somewhat augmented by such a procedure, we yet have the great subsequent advantage, should the case do well, to fall back upon. Apart from the inconveniences and dangers of enlarged prostate in itself, there can be no doubt but that the risk of a second calculous formation would not have been inconsiderable. In Mr. Fergusson's case a deep lesion was formed behind it, and in this any little fragments of stone which had been broken off would have been almost sure to remain, and might very probably become the nuclei of future ones.

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38. *Treatment of Hydrarthrosis of the Knee-joint by puncture and injection of Iodine.*—Dr. Robt. L. Macdonnell states (*The Medical Chronicle*, June, 1857) that he has treated successfully five cases of hydrarthrosis of the knee-joint by puncture and injection of iodine.

"Many," he says, "of those who object to this plan of treatment, do so under an erroneous idea of the nature of the disease for which it is recommended, and also of the results that are expected to be derived from it. Thus, we find some who have written and spoken against it, confounding simple chronic hydrarthrosis with white swelling (tumeur blanche), and objecting to the injection of a dropsical joint, because the treatment did not succeed in a completely disorganized and suppurating articulation. Now, I would wish it to be distinctly understood, that it is to *pure uncomplicated chronic hydrops* of the knee-joint, that my suggestions as yet apply; for I have not employed the treatment in any other joint, and though I do not believe that injection of iodine would do any harm to a joint already destroyed by ulceration, yet I wish the point to be clearly understood, that it is not in such affections I recommend it. It has also been stated that serious consequences must necessarily follow the mere puncture of so large a joint, and *a fortiori*, the injection of it with iodine, or any other irritating substance, must be extremely dangerous. I shall not here occupy my reader's time in proving the difference, as to consequence, between the puncture of a joint in its *physiological* condition and a similar wound inflicted upon it in its *pathological* state. Until the surgeon shall have acquired clear and distinct ideas on this point of practice, he cannot understand the rationale of injections into joints, the seats of chronic synovial effusions; nor of incisions into large and small joints already in a state of suppuration, as recommended by Gay and others, and which (as regards the smaller joints) I have myself often performed.

"There is another objection urged against this practice, founded on the supposition that excessive inflammatory action must necessarily follow the injection, and that anchylosis or even the death of the patient might ensue. In this, as in other instances, one fact is worth a hundred theories. Puncture and injection have been performed not only without bad consequences, but with the greatest success; therefore, to discuss this point any further would be fruitless. But some of the opposition to the practice may be ascribed to the views of Bonnet himself, and to his method of operating. He injected the joint, with the express object of exciting an acute arthritis, which being combated in the usual manner, led to the removal of diseased action from the articulation, and no steps were taken to prevent this excessive inflammatory process. Now, it is evident that, as in hydrocele, the radical cure is often effected, not by inducing violent inflammation of the sac, but by modifying its diseased secreting action, so we should endeavour to induce merely a slight change of action in the synovial lining of the joint; and adhere, as closely as possible, to the rule laid down by Velpeau and Cabaret to procure in shut cavities, containing effused fluid, an irritation which should be constantly adhesive and never purulent."

"By the method which I recommend, the fluid is forced upwards from the articulation to the synovial bag above the joint lying on the anterior surface of the femur, and when the fluid is withdrawn and only half an ounce of iodine solution injected, the opposed surfaces of the sac are brought into contact and retained in that position by the gentle and equable pressure of a well applied wet bandage. By this means, air is prevented entering the joint, the injected fluid is spread out evenly over the whole synovial surface, and becomes still more diluted by admixture with the secretions of the parts, and a healthy action

taking place, excess of exudation and engorgement is prevented, and it is not unlikely that adhesion occurs in the opposed surfaces of the dilated pouch in front of the femur. This result is still further favoured by retaining the joint in an immovable position by bandaging it to a strait splint, and by keeping the patient upon cooling diet. Bonnet recommends us to push the trocar down in a perpendicular direction from the front of the tumour till it strikes the femur. I see no advantage in piercing the synovial membrane in two places, and in wounding the femur; nor have I followed his other direction to inject a quantity of iodine solution, equal to the amount of fluid drawn off. In my practice, I have thrown in only two drachms of strong tincture mixed with two of lukewarm water.

"In conclusion, I would wish it to be understood, that I do not advise the above line of treatment to be pursued, except in simple uncomplicated hydrarthrosis, that has resisted all other remedies, and that has led to loss of use of the limb, or has prevented the patient earning a livelihood, and enfeebled his constitution by protracted suffering."

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39. *Tracheotomy in Croup.*—The medical officers of the *Hôpital des Enfans* have long advocated an early performance of the operation of tracheotomy in croup; and M. Andre, one of the *internes* of that institution, has just published an account of the operations performed during 1856. The following table exhibits the results according to age:—

Age.	Total.	Deaths.		Recoveries.	
		Boys.	Girls.	Boys.	Girls.
15 months to 2 years	6	2	4	—	—
2 to 3 years	9	4	3	2	—
3 to 4 "	13	5	4	4	—
4 to 5 "	11	6	3	1	1
5 to 6 "	6	3	1	1	1
6 to 6½ "	3	1	1	—	1
7 years	2	—	1	—	1
8 "	2	—	1	1	—
9 "	1	—	—	1	—
9½ "	1	—	—	—	1
	54	21	18	10	5

Thus it will be seen that a considerable proportion of the cases were successful, and that this has been so in proportion as the children have been advanced in age. In all the children of less than two years of age, the operation proved fatal; and the others who succumbed, with two exceptions, scarcely exceeded that age. In the two older children (seven and eight years of age) who died, there were other causes of death independently of the operation. The explanation of this circumstance, M. André supposes to exist in the fact that children of four years of age, who recover more frequently than younger children, offer greater resistance to both the accidents of the operation itself, such as hemorrhage and traumatic fever, to the diphtheritis, and the complicating affections, such as capillary bronchitis and pneumonia. They are also more docile, and allow more readily of the repeated examinations of the wound and canula that are necessary; while suitable diet, so essential, and so difficult of management in very young children that have been operated upon, is more easily regulated. It is probably also due to the greater power of resistance possessed by boys, that the proportion of their recoveries exceeds that of the girls. Another circumstance to be mentioned, is the deplorable facility with which children who have not already had the measles or scarlatina contract these affections upon admission into the hospital; and although, usually, eruptive fevers are uncommon prior to the fifth year, scarlatina attacked no less than ten of these little patients, of whom a third part died. M. André agrees in the justice of the opinion long held by the officers of the hospital, that the ulterior success of tracheotomy is much interfered with by the earlier employment of debilitating remedies, such as venesection, leeches, blisters, &c.—*British and Foreign Medico-Chirurgical Review*, July, 1857, from *Bull. de Thérap.*

40. *Treatment of Hydrocele in Children.*—M. Richard, while attending for M. Guersant at the Hôpital des Enfans, met with no less than twelve cases of this affection in the course of one month; and although accident may have led to this accumulation, he yet believes that it is of more common occurrence than is usually supposed.

The hydrocele of children is commonly termed congenital, and as in the great majority of cases the vaginalis communicates with the peritoneum, *congenital persistence* seems to be one of the conditions of the disease. Not that all these serous collections can be reduced by the hand, for it is more common to find them irreducible; but nearly all of them, if watched sufficiently long, are found appearing and disappearing, increasing and diminishing, from time to time. Of these twelve cases, two only were purely funicular, the ten others invading the scrotum and cord. The cysts of the cord, which often simulate a third testis, are in children and adolescents developed in the funicular portion of the persistent vaginalis; while in the adults we observe cysts of the epididymis, containing a turbid fluid and spermatozoa.

Experience has shown that, in the treatment of these hydroceles, the persistence of the communication with the peritoneum is not of the importance that might have been expected. M. Richard's cases are treated in the following manner: 1. The liquid is evacuated to the last drop by means of a short exploratory trocar, of very small calibre. 2. An assistant exerts compression upon the lower part of the belly and the track of the inguinal canal. 3. From six to seven grammes of alcohol (40° of Beaumé's areometer) are then thrown in. 4. The canula is suddenly withdrawn so as to leave the fluid in the sac; and after continuing compression over the inguinal canal for a minute, the operation is concluded. The consequences are very simple. The tumour increases a little towards evening, becomes a little painful next day, after when all pain entirely ceases. From the tenth to the fifteenth day, the tumour entirely disappears, and the child is cured. If he is of an age to admit of it, he is allowed to walk or play about after the operation. Sometimes, at the instant of withdrawing the canula, owing to the strong contraction of the cremaster and dartos, a little of the injected fluid is expelled, the little patient being at the same time seized with erection, or even expelling his feces. But this is of no consequence. Sometimes, however, a few drops of the fluid enter between the skin and the vaginalis. This is followed by redness of the skin, and the formation of a small abscess which bursts of its own accord, without interfering with the progress of the cure. The *smallest possible trocar* must be employed in this delicate operation, although, without practice, such an instrument is more difficult to use. We must render the hydrocele very tense with the left hand, isolating it as much as possible, and holding the trocar in the right hand, apply the pulp of the thumb and the index finger very near the pointed extremity of the instrument. In place of thrusting this in, as in the adult, we must insinuate its point as if using a bistoury. When the cure has been obtained, a good precaution consists in wearing an inguinal bandage for three or four months; for a principal advantage of the operation for congenital hydrocele, is derived from the protection it affords against the production of hernia.—*British and Foreign Medico-Chirurgical Review*, July, 1857, from *Gazette des Hôpitaux*, No. 41, 1857.

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41. *Enucleation Treatment of Uterine Fibrous Tumours.*—Mr. J. Hutchinson has given (*Med. Times and Gaz.*, Aug. 8 and 15, 1857) an interesting report on this subject. The following are his conclusions:—

"1. That surgical interference with interstitial fibrous tumours of the uterus is always attended with very considerable risk, and ought not to be practised except under circumstances of urgency, or when the position of the growth is peculiarly tempting.

2. That when the tumour is not of very large size, and is already partially extruded, the operation is rendered comparatively devoid of danger, and ought to be performed at once.

3. When the tumour is yet wholly imbedded, and even when of large size,

the enucleation treatment is yet warrantable if the patient's life be threatened by hemorrhage.

4. That primary enucleation, where at all easily practicable, is much preferable to the secondary method.

5. That where the tumour is very large, or where found after the incisions to be firmly united to its capsule, the secondary plan should be preferred.

6. That whichever plan it is intended to adopt, the first incision should, excepting under unusual circumstances, be made from within the cavity of the cervix, and should be as free as possible.

7. That the danger of hemorrhage from this incision is very slight indeed.

8. That as much should be done as practicable at the first operation in the way of freeing the tumour from its cyst. Thus, if the adhesion be found more loose than had been expected, a primary enucleation may be completed where the slower plan had been proposed.

9. That the after-treatment in cases of primary enucleation should consist in warding off the shock by opiates, in sustaining the strength, and in the free use of stimuli and nutritious diet.

10. That in cases of secondary enucleation, the ergot of rye should be administered, so as to keep up vigorous uterine action, that the greatest attention should be paid to sustaining the patient's strength, and to the removal of discharge and shreds of slough as fast as formed.

11. That in cases of great exhaustion and threatened pelvic inflammation, the internal use of turpentine is of great value.

12. That in cases of secondary enucleation the surgeon need not be anxious about the removal of the tumour *en masse*, but may entertain a good confidence that if it have taken on a sloughy state in its lower part that the death and piecemeal disintegration of the whole will follow.

13. That the "recurrent fibroid" tumours slough away yet more readily after interference than the true fibrous ones, although liable to return after a short interval of health.

14. That after a successful enucleation procedure, complete, though gradual, restoration to good health may be expected."

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42. *Ovariotomy.*—Dr. Charles Clay, of Manchester, states (*Edinburgh Med. Journal*, June, 1857), that he has performed this operation in 79 cases, of which 24 terminated fatally, and 55 in recovery.

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43. *Concretions of the Prostate.*—Mr. H. Thompson read before the Royal Med. and Chirurg. Soc. (June 9th) a paper on the concretions of the prostate, their mode of production, and their relation to the formation of prostatic calculi—the result of observations made upon upwards of fifty dissections of the prostate.

The existence of "concretions" of microscopic size has been established in every one of the fifty specimens of the prostate exhibited. In many, their size was that of a poppy-seed. They had been found, also, in the organ at 14 years of age. Their physical and chemical characters (the latter by rigid analysis) were given at considerable length. Their existence was concluded to be a necessary result of the performance of natural functions on the part of the prostate. After numerous observations, it appeared that the formation of a concretion frequently originated in the aggregation of a yellowish matter, often seen within the secreting nuclei lining the gland-ducts and pouches, often found free in yellowish granules, sometimes stuffing small ducts and follicles, and seen floating in the form of small prostatic fluid, as well as in the contents of the vesiculæ seminales. In the larger masses of this yellow matter, entirely occupying the interior of crypts or follicles, the small granules may be seen cohering, or as if fusing together, and presenting an appearance identical with that which is often seen existing in the centre of fully formed concretions. It was concluded that the coalescence of these yellow granules, or of the nuclei charged with them, their partial fusion into a mass more or less homogeneous, the stratification in part of this mass itself, or more probably the deposit upon its surface of fresh layers of fluid matter similar to that which originally constituted

the interior, and finally, some addition of opaque earthy matter to it, either by infiltration or accretion (through irritation of the secreting membrane around, from the pressure of the newly-formed body, as observed in numerous other instances referred to), were the steps by which the production of a "prostatic concretion" was very frequently accomplished, and its connection with "prostatic calculus" illustrated. The views of other observers were quoted and discussed at considerable length. Numerous drawings of these bodies in various stages of formation, as well as the original objects themselves, under microscopes, illustrated the communication.

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44. *Gonorrhœa of the Nose.*—Mr. Edwards records (*Lancet*, April 4, 1857) a case of this in a respectable widow, produced by her using a handkerchief for her nose, which her son, who was labouring under gonorrhœa, had employed to suspend his testicles.

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45. *New Crimean Tourniquet.*—Mr. Salt objects to the ordinary tourniquet, especially in military use, on account of the liability of the pads and straps to become saturated and clotted with blood; and to Skey's and Signorini's tourniquets on account of their complexity and costliness. He describes one devised by himself, which consists of an ordinary stay busk of sufficient length to embrace the largest limb, and by a series of notches at both extremities may be so reduced as to fit the smallest. It is coated with elastic gum, smoothly polished, so that it may be kept clean simply by the use of a sponge and warm water. The limb being embraced by the tourniquet, all that is necessary is to direct pressure upon the artery by a few turns of the screw.

New Crimean Tourniquet.

The following are stated to be the advantages of the instrument:—

1. It has no detached parts; is adapted for any sized limb; and may be applied instantaneously.

2. It yields equally to the action of the screw, concentrates the pressure upon the artery, and does not interfere with the venous circulation.

3. It dispenses with all straps, buckles, and other adjuncts; and is available at any moment, and for any number of cases.—*Assoc. Med. Journal*, Nov. 29, 1856.

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46. *Rectangular Catheter Staff for Lithotomy.*—Mr. Hutchinson read before the Royal Medical and Chirurgical Society (Feb. 10, 1857) an account of a rectangular staff for lithotomy. The main peculiarities of the instrument were its rectangular form and its catheter stem. The advantages *of its form* were, 1st, that it made the direction into the bladder straight, and thus obviated all danger of the knife leaving the groove; 2dly, that the angle projecting prominently into the perineum was more easily found than the curve of an ordinary instrument; 3dly, that its groove commencing only at the angle, there was no chance of the urethra being opened too far forward, or the artery of the bulb being wounded; 4thly, that when once introduced it did not easily change position. Its being a *catheter as well as a staff* was important; 1st, because it allowed the surgeon to be quite certain of its being really in the bladder before commencing the operation; 2dly, because it permitted of the bladder being injected without any change of instruments, and thus prevented the risk of the

water escaping. It was provided with a stopcock. The author insisted strongly on the importance of operating with a full bladder and the dangers of its neglect, and believed that one great recommendation of his instrument was, that it would much encourage and facilitate the practice. Adverting to the causes of accidents in lithotomy, he stated, that of a series (nine) which had come under his notice during the last few years, chiefly in the practice of the London Hospitals, they had been due to, 1st, the knife leaving the groove in the staff; 2dly, the staff being at the time not really in the bladder; 3dly, injury of the fundus of the bladder with the point of the knife; and expressed a strong opinion that the employment of the "rectangular catheter-staff" would have prevented them all. The instrument shown to the Society had a side groove, and was adapted for the use of any form of knife the operator might prefer. It had been made by Messrs. Fergusson, of Giltspur Street. It had been tried in the deadhouse a great number of times, and once upon the living subject, and always without any inconvenience. There was no difficulty whatever in its introduction. It was advised to be held in the usual way, moderately hooked up under the symphysis pubis, but by a slight movement of the handle its angle might be made to project more or less into the perineum, according to the operator's wish. Mr. Hutchinson wished distinctly to state that he made no claim to originality of design. Dr. Buchanan, of Glasgow, had long ago recommended and used an angular staff for lithotomy, and more recently Mr. Fergusson had devised a grooved catheter for perineal section. The present instrument was merely a combination of the two principles. He believed, however, that it possessed in its catheter stem a very important advantage over Dr. Buchanan's, since it enabled the operator to ascertain with positiveness whether he was in the bladder. With instruments of the ordinary curve this is done by striking the stone, but as an angular one is very inconvenient for sounding, it is liable, when made solid, to the objection that the surgeon might occasionally have to operate in uncertainty. The author also laid before the Society several other modifications of the angular staff which he had had made in the course of a long series of experiments as to the safest instruments for lithotomy. One of these had the groove beneath, and the knife adapted to it was a double cutting gorget, the beak of which was so made that when once placed it could not leave the groove. This he had once used on the living without inconvenience; but as it was liable to some objection, and as the side groove allowed of the operation being completed by a single knife, and much simplified the apparatus, he had at length abandoned the principle which distinguished the former.

Mr. Spencer Wells said the rectangular staff had been described eight or nine years ago by Dr. Andrew Buchanan, of Glasgow, in a Scotch journal. That gentleman laid especial weight upon the point, that in his operation the staff should be pressed downwards, and not drawn upwards, so that the rectum, instead of being drawn into danger, might be pressed out of it. The projecting angle of the staff was brought very near the anus in the median line, and the forefinger of the left hand passed into the rectum to feel the apex of the prostate, taking care that the projecting angle was immediately in front of, and not behind, the prostate. Dr. Buchanan made a very different incision from that made in the ordinary lateral operation. The knife was passed into the groove of the staff, directly in front of the anus, and held horizontally, as if the operator were going to cut towards the tuberosity of the ischium, and in withdrawing the knife it was brought out downwards, so that the incision resembled one-half of the incision made by Dupuytren in his bilateral operation, except that it was much nearer the anus. In this way Dr. Buchanan considered that the rectum could not be injured. In his (Mr. Wells's) experiments on dead subjects, he had found that unless the assistant was careful, and the surgeon also, the angle of the staff was apt to slip into the bladder, and the prostate was pushed forwards towards the perineum; and thus the bladder was opened either through the substance of the prostate or behind it, the old operation of cutting upon the gripe being thus performed. This was likely to take place unless great care was taken to feel with the forefinger in the rectum that the angle of the staff was made to rest exactly at the apex of the prostate;

in fact, that the prostate was between the finger and the horizontal branch of the staff.

Mr. ERICHSEN said that he thought Mr. Hutchinson's staff likely to prove of considerable value. He could give no opinion as to the merit of the rectangular shape, having had no experience of it, but he thought the combination of the catheter with the staff peculiarly valuable; it enabled the surgeon to inspect the bladder without changing instruments, prevented the escape of the injected fluid, that was apt to ensue between the withdrawal of the ordinary catheter and the introduction of the staff, and would tend to prevent some of the accidents that might befall a surgeon in lithotomy. Had he been provided with such an instrument, an accident that had once occurred to him might have been avoided. A patient was sent to him with stone in the bladder, which was at once detected on sounding. When about to be cut, the staff was introduced, apparently into the bladder, but no stone could be felt. A hollow sound, with a short beak, was then passed, and the calculus at once struck. The staff was again introduced, and, as it passed without difficulty of any kind, and the point could be felt on depressing the handle above the pubes, it was supposed by Mr. Erichsen and those assisting him that it was in the bladder; but that, having a large curve, the stone probably lay in this. Mr. Erichsen proceeded to cut the patient, and, on opening the groove of the staff, found it was not in the bladder. Having satisfied himself of this, he withdrew the staff, and passed the hollow sound, with which he had previously felt the stone, cut upon this, and extracted a large calculus. The patient unfortunately died, and after death three old false passages were found leading from the urethra into the abdominal space, and by the side of the bladder. It was into one of these that the staff had passed, but the sound being larger, and having a short beak, escaped it, and entered the bladder. Now, had the staff been hollow, as in Mr. Hutchinson's instrument, such an event could at once have been recognized by the non-escape of urine. Since this case had occurred, Mr. Erichsen had heard of several instances in which a similar accident had happened. This led to the inference that the proper rule of practice should be, to feel the stone with the staff rather than with a sound when the patient was on the table. He did not know whether the use of the hollow staff in perineal section originated with Mr. Fergusson or Mr. Thompson, but he had used the instrument with great advantage during the past twelve months.

OPHTHALMOLOGY.

47. *Amaurosis with Bright's Disease of the Kidney.*—The connection between these two diseases was pointed out nearly about the same time by Landouzi, of Rheims (*L'Union Médicale*), and by the editor of this Journal (*Proceedings of Philad. Coll. of Phys.*).

Mr. HOLMES COOTE, in a recent paper on this subject (*British Medical Journal*, April 25, 1857), remarks:—

"When the kidneys are so diseased that the proper urinary excretion is impaired, and the effect is felt in the general constitution of the blood, the walls, not only of the larger arteries, but likewise of the capillaries, undergo a molecular or granular degeneration, by which they lose their elasticity and strength, and are very liable to give way. In the examination of the bodies of persons who have died under these circumstances, I have found in the brain four, or even more, small extravasations of blood in the various stages between recent effusion and complete repair. The same morbid changes occur, I believe, in the arteries of the choroid, retina, and vitreous humour, where they give rise to changes which impair or destroy vision, and may be recognized during life."

"Although Blödig found only three cases of this combination of amaurosis and Bright's disease of the kidney during the five years he studied in the

Ophthalmic Clinic of Vienna, I believe it will be found of more general occurrence, not, perhaps, always manifested by sudden loss of vision from the rupture of diseased capillaries and the effusion of blood, but by changes dependent on the disturbed circulation through vessels which have lost their elasticity and power of regulating the flow of blood through tissues perhaps the most delicate in the human body—changes which, though slow, are not the less sure to terminate in the disorganization of those parts upon which depends the susceptibility to the impressions of light. Thus, Dr. Heymann, of Dresden, found, in one case of similar nature, turbidity of the vitreous humour; in another, a sort of serous swelling of the optic nerve papilla; in another case, the retina was covered 'as if by a cicatrix.' I found, in a case recently examined, loss of brilliancy in the choroid, absence of defined arrangement of the arterial capillaries. The vessels brought into view appeared to be venous, and were tortuous and varicose."

48. *Diphtheritic Ophthalmia.* By MM. WARLOMONT and TESTELIN.—This article constitutes one of the additional chapters its authors have contributed to their translation of the last edition of Mackenzie's treatise *On Diseases of the Eye.* A form of pseudo-membranous ophthalmia was, they say, indicated by Bovisson, of Montpellier, in 1847; and M. Chassaignac has alluded to the pseudo-membranes which occur in the ophthalmia of new-born infants; but the present affection has been particularly described by Gräfe, of Berlin, the disease, indeed, thus far, seeming peculiar to Germany.

Symptoms.—In an eye in its normal state, but more frequently in one that has already suffered from inflammation, great irritation is suddenly set up, accompanied by much tumefaction of the conjunctiva, an inconsiderable amount of chemosis taking place. A network of large vessels ramifies over the conjunctiva, and the membrane itself, of a yellowish colour, is marbled over by a great number of minute red points. If an incision be made into the chemosis, fluid does not flow out, the submucous tissue being infiltrated with gelatiniform fibrin. The upper eyelid is remarkably tense, and so much pain does its eversion produce, that Gräfe resorts first to chloroform. When the conjunctiva is thus exposed, it presents a polished yellowish colour, which is due to a fibrinous exudation that penetrates into its substance, and leads to arrest in the circulation. The exudation can only be detached from the membrane with difficulty. It may be well seen by causing the patient to look downwards, and by everting the superior eyelid sufficiently to see the oculo-palpebral fold. The lower eyelid is also rigid, but little movable, and very red. Besides the symptoms mentioned, there are two others—a great and continuous increase of temperature, and an abundant discharge of a dirty gray fluid, carrying with it yellowish flocculi. This condition, which constitutes the *first* stage of the affection, continues for a longer or shorter period; and the diphtheritic process may still be going on after the original swelling of the eyelid has diminished.

After awhile, the rigidity of the eyelids disappears, and the conjunctiva assumes a spongy appearance, abundant fibrinous masses becoming detached from its surface. In parts, it may retain its habitual colour, while in other places, the exposure of the vessels gives rise to copious bleedings. The portions deprived of the mucous membrane swell more and more, and assume an appearance very like that seen in chronic blennorrhœa. There are also numerous nodosities formed upon portions of the conjunctiva, which, resistent at first, soften with the progress of the affection. The chemosis of the ocular conjunctiva now loses its hardness and yellow colour, and a dense vascular network is developed, so that the diagnosis of the affection at this period is very difficult.

The third stage is characterized by retraction of the eyelid, proportionate to the amount and depth of the original fibrous infiltration. In some patients, day after day false membranes are removed three-quarters of a line in thickness, and having a hole in the centre corresponding to the circumference of the cornea. These are the pathognomonic signs of the affection; but to them may be added various degrees of opacity or ulceration of the cornea.

Nature and Causes.—As to the nature of the affection, it is derived from an

inherent disposition of the mucous membrane to take on the diphtheritic action. It is a general disease, occurring more frequently in unhealthy than in healthy individuals, and internal affections frequently prevail during its progress. Thus, among forty children, the subjects of it, M. Gräfe found death result in three from croup, and in other instances from pneumonia or hydrocephalus. It also frequently coincides with diphtheritic inflammation of the skin or apertures of the mucous membranes. Eight of these forty children were the subjects of congenital syphilis. When one eye is affected the other often suffers, whatever precautions may be taken to prevent inoculation. Epidemic influences are its principal cause, for, after months have elapsed without a case occurring, great numbers may be suddenly met with. New-born infants are not especially predisposed to it, although in the ophthalmia they suffer from it; a fibrinous exudation, giving a certain amount of rigidity to the eyelid, may be observed. The affection is indubitably contagious. As already existing inflammation, especially when traumatic, predisposes to it, operations must be abstained from when the affection prevails epidemically.

Diagnosis and Treatment.—The only affection it can well be confounded with is gonorrhœal ophthalmia, and the author exhibits the distinctive signs at some length. With respect to treatment, copious depletion, by means of leeches applied near the angle of the eye, but especially to the root of the nose, is recommended by M. Gräfe. Ice-cold affusions are also to be frequently applied, and the eye is to be kept scrupulously clean, for which purpose milk is one of the best appliances. In certain forms of the disease, caustics may be required. M. Gräfe strongly recommends the energetic employment of mercury, this being the only internal medicine of any value. The regimen must be strict, and but little fluid should be taken. In several cases, the second eye has been preserved from an attack by keeping it closed.—*B. and F. Med.-Chir. Rev.*, July, 1857, from *Annales d' Oculistique.*

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49. *Photophobia and Blepharospasm relieved by Chloroform.*—Dr. Mackenzie, of Glasgow, communicated to the Royal Med. and Chir. Soc. a case of intense and long continued (sixteen months) photophobia and blepharospasm relieved by the inhalation of chloroform administered seven times. Mr. Arnott stated that he had administered this remedy in a case of strumous ophthalmia, with intolerance of light, with not only immediate but permanent relief.

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50. *Hemorrhage from the Eyeball after the Extraction of Cataract*—Mr. White Cooper relates (*Lancet*, April 11, 1857) the following very interesting examples of this unusual occurrence.

Case 1. On June 13, 1855, I performed extraction on the right eye of Mrs. B., aged seventy, who had long attended as an out-patient. Each eye presented a hard, amber-coloured cataract. The globes were hard and rather prominent; anterior chamber full; irides natural in colour; pupils ordinarily motionless, but dilating under atropine. She could distinguish light from darkness, and the shadow of a hand. Some years previously she had suffered from deep-seated pain in the eyeballs, with muscæ, and had long been teased with irregular gout.

From the hardness of the globes, and immobility of the pupils, I thought unfavourably of the case; but a woman's earnest entreaties that I would, at all events, give her the chance afforded by an operation, induced me to make the attempt on one eye. The section was completed with facility, the curette used, and with gentle pressure the lens escaped, a small quantity of rather fluid vitreous humour accompanying it. The flap being replaced, the lid was dropped, and I paused before taking a final look at the eye. After a few seconds, the patient clapped her hand to her eye, and exclaimed, "Oh, what a pain in my head!" Removing her hand, I looked at the lid, and saw it rapidly becoming distended; gently raising it to ascertain the cause of this unlooked-for circumstance, I found the section gaping widely, and a large mass of hyaloid pushing through it; in a second or two, a gush of blood poured from the eye, and flowed down the cheek.

I at once recognized the case as one of those generally described as rupture of the central artery of the retina, though I may here mention that, judging from the great flow of blood, and bearing in mind the small size of the vessel in question, I regard the choroid as the seat of the hemorrhage; it was precisely such a discharge of blood as I have seen when the choroid has been wounded in removing a staphyloma, and very obstinate the bleeding may be under those circumstances.

The remedy which I have found most useful in checking hemorrhage from the eye is pounded ice, folded in linen and laid on the lids. Little can be done in the way of pressure, the parts being so exquisitely sensitive that it cannot be borne. Pounded ice was, therefore, applied to this patient's eye, and three grains of gallic acid ordered to be taken every four hours. Notwithstanding the steady perseverance in these measures, the bleeding continued for thirteen hours, when it gradually ceased.

On the evening of the 17th, the patient being low and faint, a cordial opiate was administered; yet, when the power of the circulation returned, to our mortification, the bleeding recurred. Ice was again applied, and in half an hour the flow of blood finally ceased.

The application of cold was persevered in for two more days, when, suppuration of the globe commencing, poultices were ordered. The patient, who had been much reduced by the loss of blood, now required liberal diet and wine, and opiates were prescribed to allay the pain accompanying the inflammation and suppuration of the globe. Under this treatment, the eye gradually became quiet and ultimately collapsed.

The second case occurred during the last winter, and was remarkable from the hemorrhage not taking place till ten days after the extraction.

Case 2. The patient was a feeble old woman, who had been a great sufferer from rheumatism, and was so extremely deaf that it was with difficulty she could be made to comprehend a single sentence addressed to her. This infirmity made her very nervous and unsteady during the operation (which, you may remember, was performed on the left eye). From her extreme restlessness the section was made with difficulty, and the iris unavoidably wounded, though not to any extent. The lens was extracted with facility, and there was no escape of vitreous humour. I observed that the cut at the margin of the pupil bled more freely than usual, but did not attach much importance to this. Cold water dressing was applied to the eye for twenty-four hours, after which dry dressings were used. The patient was allowed liberal diet on account of her want of power. Being anxious that every opportunity should be afforded for firm union of the section, the eye was not disturbed till the seventh day. On that day, however, observing a little œdema of the upper lid, I opened the eye, remarking that there was probably serous chemosis and non-union. There was, however, then, no chemosis; the eye was pale, cornea clear, but the section not united. I decided on closing the eye again for some days, and ordered wine, with bark and ammonia. Examining the eye again on the tenth day, the section was still open, and serous chemosis existing; a solution of nitrate of silver was directed to be carefully applied once a day.

That evening the sister of the ward was suddenly called to this patient, and, to use her own expression when describing it to me, "the eye appeared to be hanging out on the cheek, with blood streaming from it." Mr. Staples was with the patient in a few minutes, and applied ice and a compress. It was evident that hemorrhage had taken place within the globe, pushing out the vitreous humour, and then escaping through the section. The hemorrhage continued actively for about an hour, then gradually ceased, and did not return. Acute inflammation of the globe, with great swelling of the lids, followed, and the patient became so prostrated that I was in some apprehension as to the result. The poor woman had so fixed her mind upon dying from the operation that, before entering the hospital, she had had her coffin made and her funeral arranged. Happily, her forebodings were not realized, and, with great care and attention, she gradually rallied, the eye sank and became quiet, and she left the hospital convalescent.

The question now presents itself, are there any means of diagnosing these

hemorrhagic cases before operation? Gentlemen, you will do well to regard with suspicion eyes presenting the following conditions: Tense, hard globe, traversed by purple tortuous veins; sluggish or immovable iris; with, perhaps, one or two minute points of adhesion to the capsule of the lens; the existence of motes, flashes, and occasional dull aching at the back of the eye, with pains of the brow and cheek, the patient, at some time or other, having had gouty symptoms. You must bear in mind, however, that the pain, the muscæ, and other symptoms of disturbance, may have passed away, and will not be mentioned unless inquiry be made: yet their existence, in connection with the other symptoms, often indicates a varicose state of the choroid and of the retinal vessels, with, perhaps, degeneration of their coats.

Dr. Gairdner has pointed out that venous congestion is a common attendant on gout; my own observation coincides with this. A patient of mine, who lost his right eye from arthritic glaucoma, besides being a martyr to the gout in his limbs, underwent an operation on the throat; uncontrollable venous hemorrhage took place, and he died. A few weeks since, I removed an eye from a patient of Dr. Gairdner's; though the ball itself was cleanly dissected out by means of scissors and a strabismus-hook (a proceeding which is generally almost bloodless); there was profuse hemorrhage at the time, and the bleeding did not cease for three hours, in spite of the constant application of ice; the patient, however, recovered so rapidly that an artificial eye was introduced on the seventh day, and borne perfectly well. I mention these cases because I believe that when bleeding does take place from eyes which have been the seat of chronic arthritic inflammation it is likely to be obstinate.

What is to be done to check the bleeding? Pounded ice to the lid, gentle pressure if it can be borne, and gallic acid internally, are the best measures. If the globe fills with blood it will be utterly destroyed; but cases are recorded where the bleeding has been slight, and recovery of sight has taken place. It is, therefore, very important to check it promptly. If the patient be old and feeble, and the powers of life failing, stimulants, as brandy, may be absolutely necessary; but the less the better. The very depression of the circulation may be the means of arresting the hemorrhage, whereas a too hasty administration of stimuli to keep up the pulse may be the very means of defeating the main object. The room should be kept cool, the patient be carefully watched and kept absolutely quiet—the fewer persons admitted the better; the alarm and agitation of friends are an annoyance to the surgeon, and are hurtful to the patient.

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51. *Ultimate Ill Results of the Depression of Cataract.*—An instructive case has recently been under Mr. Bowman's care in the Moorfields Ophthalmic Hospital, in which depression had been practised with perfect success, and, after nearly two years of good sight, the eye had been lost by inflammation. The particulars are briefly as follows: George H., now aged 62, came up from the country to be under Mr. Bowman's treatment for cataract, in May, 1854. The cataract was most advanced in the left eye, in which it had existed for about eighteen months. He was blind of the left, and the right was becoming inconveniently misty. Mr. Bowman (who was then performing depression in a number of cases, with the view of testing the value of this operation as compared with extraction) depressed the lens in the left eye, and with perfect success. The man soon after returned home, and for more than two years enjoyed sight which enabled him to read the smallest print, and had no pain whatever in the eyeball. In July, 1856, he was suddenly attacked by severe pain in the globe, for which he could assign no cause. He describes the illness as having been attended by much pain and feverishness, and within a week of its commencement had lost his sight. After a time, the pain subsided to a great extent, but he remained without any vision whatever in the affected eye. In the mean time, the cataract in the right eye had so much increased that he could make but little use of it. In May, of the present year, therefore, he came in order to have a second operation performed. The lost eye was constantly fretful and painful, and the lashes of the lower lid were turned against it. Before proceeding to extract, Mr. Bowman first performed the usual opera-

tion for entropium, and also determined to remove the lost globe, fearing that it might prove a source of irritation to the other. This was accordingly done, and an opportunity thus occurred of examining the state of the organ. The nucleus of the depressed lens was found still existing, and, although dense and shrivelled, was not actually cretaceous. The choroid and retina had been separated by effusion between their layers. Last week, Mr. Bowman extracted the lens from the right eye, and when the eye was opened, a day or two since, the section was found united, with a central clear pupil. The man has been wearing an artificial eye with perfect comfort, from the fifth day after the excision. The readers of our notes will be aware that the operation by depression is one which is now never performed at the Moorfields Hospital. It has been wholly discarded on account of the frequency of such occurrences as the above case illustrates, viz., that the nucleus of the lens is liable at any subsequent period to become a source of irritation and excite inflammation, which will end in amaurosis. Although so much easier of performance, and so tempting on account of its little risk and the speed with which good vision is obtained, it is therefore, in the long run, a much less satisfactory procedure than extraction. We might add that the operation by solution, when the lens is hard, is liable to nearly the same objection. The outer parts of the lens only are really absorbed, and, after a succession of troublesome operations, the nucleus at last drops out of the axis of vision, and a condition of things very similar to that obtained by depression exists, and one which is liable to the same subsequent risks.—*Med. Times and Gaz.*, June 27, 1857.

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52. *Symblepharon treated successfully by Blandin's Method.*—Mr. R. Taylor relates (*British Medical Journal*, June 13, 1857) the case of a lad, 15 years of age, who applied for relief at the Central London Ophthalmic Hospital in August, 1856. "Nearly a year and a half previously he had received a severe injury in the right eye, and a less serious one in the left, from lime, for which injuries he had been treated for some months at a hospital. His state, when I first saw him, was as follows: The right eyeball and lid were united by a dense firm cicatrix, extending nearly the whole length of the palpebral fissure, and upwards over the cornea so as to conceal the pupil, with the exception of a small chink, which was obscured by a faint milky opacity. Every attempt at moving the eye was attended by a most disagreeable feeling of constriction; and the sight was so far impaired that he could not make out the largest print. In the left eye the consequences of the injury were so slight as not to require surgical interference. There were a few loose bands of adhesion at the bottom of the palpebral sinus; and though the lower part of the cornea was obscured by opacity, this did not trench upon the pupil, and vision was unimpaired.

"The operation upon the right eye may be described nearly in M. Blandin's words. The cicatrix was carefully dissected from the cornea and sclerotica in its whole extent, until the eyeball was completely free; and the dissection was continued until the bottom of the palpebral sinus had been reached, and the bony margin of the orbit could be felt with the finger. There was thus left a broad thin flap of cicatrix adhering to the tarsal margin of the eyelid. This was then folded in, in the manner of a hem, so that its smooth surface, which had formerly been external, was now in apposition with the raw surface of the eyeball, and a lining was provided for the eyelid to the very bottom of the palpebral sinus. In this position the flap was retained by two sutures passing through the entire thickness of the lid, and tied externally on the skin of the face.

"The subsequent progress of the case may be thus condensed: One of the sutures was, unfortunately, detached by the efforts of the patient, who did not understand its importance; and I was not informed of the accident until it was too late to replace it. The other was removed on the fifth day after the operation. The outer three-fourths of the flap adhered to its new position, leaving a palpebral sinus of the natural depth. At the inner part there was still a narrow elastic band between the eyelid and the lower and inner part of the cornea; which, however, did not interfere with the free movement of the eye. A subsequent attempt to get rid of this adhesion was unsuccessful. The opacity

which formerly covered the cornea has been removed, partly by operation and partly by absorption, to such an extent that three-fourths of the pupil are now perfectly clear; and the vision is almost, if not altogether, as good with this eye as with the other. So little irritation was produced by the operation, that the lad returned to his work (that of a plasterer) the day after the second suture was removed.

"Although, owing to the unfortunate detachment of one of the sutures, the success of this operation has not been so complete as it might otherwise have been, still the relief afforded in a case which is universally condemned as hopeless has been so striking that I earnestly trust that this very ingenious and simple operation may have a further trial in this country; this, so far as I am aware, having been the only occasion on which it has been performed. Among the labouring and manufacturing population, who are in an especial manner exposed to the accidents by which symblepharon is produced, there must be many who are suffering from all its inconveniences, who would gladly avail themselves of any chance of relief by surgical means, and who might perhaps ere now have been relieved, had this mode of operating been known and attempted. To speak confidently, from the results of two operations (M. Blandin's and my own), would be premature. All I would wish to enforce is, that the method proposed appears to be founded on just and scientific principles; that it is the only one hitherto proposed which is at all likely to be adopted in this country for the relief of a very distressing affection, hitherto considered as irremediable; and, finally, that it has proved successful in the only two instances, so far as I am aware, in which it has been performed."

[Where the adhesion is extensive, and the bands long, this operation may be successfully resorted to; but we have met with cases where the lid and ball were so closely united that no flap could be made. In a case in which there was only one narrow connecting band, we, several years since (1843), divided the band, and united the edges of the cut on the inner surface of the lid by suture, with the most satisfactory result. Mr. Wilde has since performed the same operation, but brought together the edges of the conjunctiva, on the eyeball as well as on the eyelid, by sutures. The former, we are persuaded, is unnecessary.—Editor.]

MIDWIFERY.

53. *Use of the Speculum.*—Dr. Robert Lee read before the Royal Med. and Chirurg. Soc. (June 9, 1857) some further observations on the use of the speculum in the diagnosis and treatment of uterine diseases. He referred to the tabular statement of 220 cases of real and imaginary disease of the uterus, published in the 38th volume of the *Medico-Chirurgical Transactions*, and presented in a similar tabular form the details of eighty additional cases which had since come under his observation. Of the 300 patients, 47 were unmarried; one had barely completed her 18th year, several were under 20, and the majority under 30 years of age, and were suffering from hysteria, leucorrhœa, dysmenorrhœa, or some nervous affection of the uterus, without inflammation, ulceration, or any structural disease or displacement of the organ. In Case 256, the patient had been told that the womb was prolapsed and much ulcerated, and an instrument had been introduced for six weeks, with an aggravation of all the symptoms. The hymen was found so perfect on examination that it was impossible to reach the os uteri without using an unjustifiable degree of violence. On the ground of morality, and on every other ground, he could see no defence for the employment of the speculum in these 47 cases. Of the 300 patients 70 were barren, and the sterility was not removed nor the other symptoms relieved in a single instance. Several of these individuals spoke with horror and shame of the treatment to which they had submitted. A considerable number of the cases were suffering from cancerous disease, in all of which the symptoms seemed to have been aggravated by the treatment. In Case 236

the character of the disease was unmistakable, but after an examination with the speculum a favourable prognosis had been given, and the actual cautery employed for months, and hopes of recovery held out to the last. The author expressed his conviction, that neither in the living nor in the dead body had he ever seen a case of simple ulceration from chronic inflammation of the os or cervix uteri, and to apply the term to states of the os uteri in which the mucous membrane, or, as it is termed by some, the basement membrane is not destroyed by ulceration, was an abuse of language calculated only to deceive and mislead the members of the medical profession, from whom the truth has been carefully concealed. The speculum emanates from the syphilitic wards of the hospitals at Paris, and it would have been better for the women of England had its use been confined to those institutions.—*Med. Times and Gaz.*, June 20, 1857.

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54. *Statistics of Coiling of the Funis.*—Dr. Weidemann states that among 28,430 deliveries the funis has been found coiled around the child in 3379 instances. In 3230 of these it was coiled around the neck, and in 149 around other parts of the body. Of the 3230 cases, 2546 consisted in a simple coil, and in 684 there were several coils. In relation to the causes of this occurrence, it is interesting to notice that of 1788 cases occurring at the Marburg Midwifery Institution, the funis was in 80 (1 : 22.2) under 15 inches, and in 183 above 25 inches (1 : 9.71) in length; that in 54 (1 : 33) there was very little liq. amnii, and in 41 (1 : 43.6) there was very much; in 165 (1 : 10.8) the child was under five pounds weight, and in 28 (1 : 61.7) it was above eight pounds. Therefore, among the favouring causes of the occurrence may be mentioned a long funis, abundance of liq. amnii, and a small child.

Among 2930 infants born at Marburg 182 (1 : 16.09) were dead, and 251 (1 : 12.41) were stillborn. Of 725 born with coiled funis, 45 (1 : 16.11) were dead, and 72 (1 : 10.06) were stillborn. Of the 45 dead-born, in 18 only could the death be referred to this alone, *i. e.* only 1 : 40.2 in the 725 examples of coiling. From an examination of the figures derived from the midwifery institutions at Dresden, Göttingen, Würzburg, Berlin, and Marburg, it results that of 13,720 new-born infants, 902 (1 : 15.21) were born dead; while in 1217 instances of coiling of the funis 31 children were born dead, whose death could be attributed to that circumstance, giving a proportion of 1 : 39 to the coilings, and 1 : 19 to the number born dead.

Thus, as (1) the 16th child among new-born children in general, as well as among those in which coiling has taken place, is born dead; as (2) the 12th child among the new-born in general, and the 10th among those around whom the funis is coiled is born stillborn; and as (3) in 1 child in 40 only can this coiling be regarded as really the cause of death, it is evident that this accident does not occupy a very prominent place.—*Med. Times and Gaz.*, July 18, 1857, from *Monatschrift für Geburtskunde*, Bd. viii.

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55. *Statistics of Placenta Prævia.*—Dr. Schwarz, of Fulda, in Hesse-Cassel, having heard the frequency of placenta prævia stated in a medical society as far greater than he had hitherto believed it to be, examined in reference to this point the official returns made by the Hesse practitioners. These were supplied by 150 accoucheurs, during a period of 20 years, *i. e.* from 1835 to 1854 inclusive. They related to 519,328 births, and among these were only 332 cases of placenta prævia—the numbers varying from 8 to 28 per annum. Of these 332 cases, 246 women recovered and 86 died: 251 children were born dead, and 85 were born living. In 40 instances the women were primiparous, and in 292 pluriparous. Podalic version was performed in 259 cases, and cephalic version in 7; while in 23 instances the children were removed by the forceps, in 6 by craniotomy, and in 13 by post-mortem Cæsarean section. In 8 instances the placenta was removed, and in 16 the plug was resorted to.—*Med. Times and Gaz.*, May 30, 1857, from *Monatsch. für Geburtskunde.* Bd. viii.

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56. *Sudden Death after Parturition, with Air in the Veins.*—By Geo. May, Jr., Esq. That death may result from the entrance of air into the veins during

surgical operations, has long been known to the profession; but that it might be a cause of danger after parturition (as suggested by Legallois in 1829), did not obtain the notice it deserved, until Dr. Cormack read a paper on the subject before the Westminster Medical Society in 1850. I propose to allude briefly to the cases narrated by Dr. Cormack, and then to give the details of three that have occurred in this neighborhood.

In 1841, Dr. Bessems attended a labour, in which there was hemorrhage with retention of placenta. On the fourth day after her confinement, whilst an injection was being thrown into the uterus, she suddenly exclaimed that she was suffocated, and died in three minutes. Air was found in the heart and veins.

M. Lionet, of Corbeil, attended a lady, aged 27. She was much frightened during the last month of pregnancy, and did not completely recover her strength; but her labour was natural, and not attended with hemorrhage. She soon, however, became faint, breathed with difficulty, and expired five hours after delivery. Air was found in the heart and in the cerebral veins.

Dr. Wintrich, in 1848, published a case of rapid death after parturition. Convulsive movements and suffocation followed the expulsion of the infant and partial separation of the placenta. Air was found in the venous system.

Professor Simpson mentions a case in which death occurred a few hours after a delivery, accompanied with hemorrhage and alternate contractions and relaxations of the uterus. Air was found to have entered through the uterine veins.

Dr. Lever mentions three cases; in all of them there was hemorrhage, and death a few hours after labour. Air was found in the uterine and other veins.

In 1850, Mr. Berry, of Birmingham, attended a primipara, aged 22. There was little hemorrhage, and she appeared to be going on well for six hours; she then became affected with difficulty of breathing and faintness, and expired in less than an hour. Air was found in the heart. The uterine veins were patulous.

Case I.—The case of which an abstract is here given, was read before the Reading Pathological Society, by Mr. Taylor, of Wargrave.

In September, 1841, Mrs. ——, aged 30, was taken in labour with her third child. The labour progressed naturally; but no urine having been passed, Mr. Taylor was in the act of introducing a catheter, when a severe pain occurred. The liquor amnii was discharged to the amount of three-fourths of a pint. The woman suddenly exclaimed, "Oh! how faint I feel," was convulsed for a moment, and then expired. By the last pain, the head had been forced partly from the outlet. An attempt was made to remove the child without success.

A *post-mortem* examination was made forty-eight hours after death. The uterus extended above the umbilicus. The placenta occupied the anterior surface from pubis to umbilicus; no portion was separated. A few days before her labour, she had a copious discharge of blood. There was little blood in the uterus. The bladder was empty. The lower vena cava was empty. The heart was healthy. The right auricle was thin, almost transparent, and distended with air. Hardly a trace of blood existed in the heart. The brain and membranes were healthy. In the spine, between the theca and the cord, there was considerable effusion of fluid blood, but none within the sheath.

Case II.—I am indebted to Mr. Smith, of Whitchurch, for the details of the following case:—

Mrs. T., between 38 and 40 years of age, was confined of her sixth child, a male, on the morning of May 7, 1852, about 8 A. M., and her attendant left her shortly afterwards, as he said, very comfortable. As, however, she had severe after-pains, an opiate was sent her. Mr. Smith was summoned to her about 2 P. M.; and on his arrival, he found she had just died. She complained of excessively severe after-pains, together with great oppression about the chest, and feelings of sinking and exhaustion and extreme restlessness. In answer to inquiries as to whether there had been any hemorrhage, the attendants stated that there had not.

A *post-mortem* examination was performed the same evening, the body not being quite cold. The abdominal viscera were all free from disease. On

opening the uterus, which was large, there was found a considerable quantity of coagulated blood; but not by any means enough to satisfy one that loss of blood was the cause of death. The uterus contained also a considerable piece of the placenta adhering to its internal surface. In the chest were old adhesions between the pleura costalis and pulmonalis. The heart appeared distended; not that it was enlarged, properly so called, but that it had an appearance of distension, which was evidently on the right side of the organ. On opening the right auricle, a quantity of air escaped with a sort of little puff, and the organ was at once reduced to its proper dimensions. No disease was found in its substance or valves. The left ventricle contained a small clot.

Case III.—In the autumn of 1855, Mrs. E., aged 28, was delivered of her third child, after a natural labour. She had become sufficiently convalescent to resume her household duties; but on the eighth day, she was taken suddenly ill, and expired before Mr. Walford arrived.

I assisted at the *post-mortem* examination the following day. No unusual appearance was observed, until the liver was sliced; it was then noticed that frothy blood escaped, and, a further examination being made, air was discovered in the vena cava inferior and vena portæ; and the right side of the heart was distended with frothy blood. The uterus was of its usual size for the eighth day. There was no sign of decomposition about the body.

Remarks.—It does not appear to be generally admitted that the entrance of air through the uterine sinuses can cause death; but if we recollect that Dr. Cless, of Stutgard, examined the bodies of 1,200 patients, who had died of various diseases, without finding air in the heart; that in the eleven cases here alluded to, death was more or less sudden, and could be explained by no *post-mortem* appearances; that the development of gas from putrefaction was quite out of the question, some of the bodies being warm at the time of examination; and that these cases present an analogy with those in which air enters the veins during operations and experiments; I think we are forced to the conclusion that the entrance of air through the uterine veins was the cause of death.

I will, however, shortly send some examples to prove that the local generation of air may in some cases prove fatal.—*British Med. Journal*, June 6, 1857.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

57. *Partial Paralysis of the Extremities, caused by the continued Use of Snuff containing Lead.*—In the year 1854, Dr. Moritz Meyer, of Berlin, published a case of lead paralysis, caused by the long-continued use of snuff containing lead. In this case he employed electricity both as a diagnostic and a therapeutic means.

The subject of the disease was a furrier, aged 38; he had experienced weakness of both hands and inability to move them well; but for three months before he was seen by Dr. M. he had completely lost the power of extending the hands. He was otherwise healthy, but his complexion was of a sallow hue. The paralytic symptoms were not preceded by any abnormal sensations except slight dragging pains of both shoulders. A powerful electric stream directed along the course of the extensors did not throw them into action. The electro-muscular sensibility of the paralyzed parts was diminished, so much so that a strong current was scarcely perceived.

The treatment employed in this case, which was clearly one of lead paralysis, consisted in the protracted use of electricity, sulphureous baths, and saline purgatives. The recovery was slow, but the cure was complete. The use of the snuff was abandoned as soon as the cause of the disease was discovered.

The snuff was (Pariser No. 2, manufactured by Bernard of Offenbach) packed in lead, and the patient, who purchased it by the pound, preserved it in the leaden envelop.

As far as Dr. Meyer knows, no case resembling this has been since communicated, and having since encountered three others he considers it his duty to make them known, believing that the chronic lead poisoning is not unfrequently produced in the manner alluded to.

The second case was a man aged 43, who, since 1845, had been in the habit of taking snuff packed in lead (Pariser No. 3). After having suffered for months from painful distension of the abdomen, especially in the umbilical region, he was seized in December, 1851, with a severe attack of colic and constipation, which lasted for six days. The like symptoms returned; the colour of the skin assumed a yellowish tinge, and great difficulty of digestion existed. In 1852 and 1853 the gentleman was sent to Carlsbad, but soon after his return in October he was again attacked by colic—again in the following February, and this attack was accompanied with paralysis of the first two, and afterwards of the other fingers of the right hand, and finally of both arms. He was first seen by Dr. Meyer in the July of 1855; he was then emaciated, anæmic, and of a yellowish waxy hue. The gums had receded from the upper teeth. The muscles of the arm were wasted. Extension was impossible. The electro-muscular contractility and sensibility of the extensors remarkably diminished. After the employment of sulphureous baths, saline purgatives, and the daily use of electricity for six weeks, there was a manifest improvement. The patient was able to raise his arm from the shoulder-joint, but extension of the wrist was still impossible. His appearance and his spirits had also undergone a favourable change. The power of the muscles gradually returned, and he was able to write. In the month of May, 1856, recovery was complete.

The third case was that of an advocate, in whom the paralysis was far more advanced than in the others. He was born in 1814, and at the age of 27 he began to take snuff abundantly. According to his own statement, he used "Pariser No. 3" uninterruptedly, which he preserved in the leaden envelop for the purpose of keeping it moist. After five years (1846) gastric symptoms appeared with fever, distension of the abdomen, pain with colic, and obstinate constipation. He also suffered from loss of appetite and want of sleep.

The skin and the sclerotic gradually assumed a yellowish colour, the hands began to tremble, and in the year 1851 this increased so much that he could not write.

In the autumn of 1852, after an attack of colic of great severity, both upper extremities became completely paralyzed.

In the year 1854, after another severe attack, the lower extremities also were affected in the like manner, having previously been affected with spasms. The Marienbad waters, &c., had the effect of removing the paralysis of the lower extremities, and the following year having learned the injurious effect of the snuff he ceased to use it. Nevertheless, in July, 1856, he presented a miserable appearance; the muscles of the arm were lax; the forearm like that of a skeleton; extension of the wrists and fingers impossible, the latter could be separated only very slightly from the palms of the hands. The face presented the aspect of saturnine poisoning. The electro-muscular sensibility was diminished in the extensors.

In the course of six weeks, during which the paralyzed muscles were daily electrified, the patient was able to extend the wrist-joints, especially the right, and was able to separate the fingers from the palm of the hand to the extent of three or four inches; the reaction of the paralyzed muscles had materially improved. At this period the patient passed from under Dr. Meyer's observation.

The fourth case was presented in the person of a gentleman formerly himself a practising physician, Dr. Kaehler, aged 45, who for the first time suffered six years since from frequent pains in the recti muscles, with painful distension of the abdomen, and a sensation in the left hand like the creeping of ants. This patient presented all the symptoms noted in the former cases in a high degree, but the constipation was more obstinate than in any of the other cases. He derived such benefit from iron baths and from the use of the Driburg waters, that he expected a lasting cure by these means, but the improvement was only temporary—the symptoms returned. However, on giving up the use

of the snuff, and having recourse to sulphureous baths and the Bitterwassa of Pülna for six weeks, he experienced a marked improvement. The steady application of electricity completed the cure in this case also. The blue line of Burton was observed in the gums of this gentleman.

Dr. Meyer observes that in these four cases of saturnine poisoning by snuff there existed—1, perfect integrity of the supinators, with more or less complete paralysis of the extensors; 2, the arching forward of the metacarpal bones; and 3d, the yellowish faded hue of the face. In three cases repeated attacks of colic preceded the paralysis; in one they were entirely absent. In three cases the common extensors of the fingers had chiefly suffered; in one the deltoid.

The four persons whose cases are detailed had all taken the snuff which is most in use, "Repé der Gebr. Bernard in Offenbach."

It now remained to prove the presence of lead in their snuff and compare it with others. The examination was performed by Simon of Berlin.

The result proved the presence of lead in the snuff, 0.78 to 1.78 per cent.; it also appeared that the intervention of blotting-paper did not prevent the snuff becoming impregnated with the lead; moreover, that the use of tin-foil (*staniol*) was no protection, inasmuch as it frequently contained lead.

The addition of a solution of common salt, which is used for the purpose of moistening the snuff, promotes the solution of the lead and consequent contamination.—*Dublin Hospital Gazette*, July 15, 1857, from *Archiv für Pathologische Anatomie und Physiologie und für Klinische Med.*

[Another case of slow poisoning by lead contained in snuff, will be found recorded at p. 406–7 of this number.]

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58. *Death from Amylene.*—Dr. J. Snow records (*Med. Times and Gaz.*, Aug. 8, 1857) the particulars of a second death from amylene. It occurred in St. George's Hospital, July 30th, in a case in which "Mr. Cæsar Hawkins removed a small epithelial tumour from the back. The patient, a short, muscular man, was a tailor, 24 years of age, who had been in the hospital several months, and had had three similar tumours removed, by as many operations, under chloroform; the last of these operations having been performed three weeks previously. He inhaled the amylene without any difficulty; in about two minutes he appeared to be unconscious, and, in another minute, the sensibility of the margin of the eyelids was somewhat diminished, and I told Mr. Hawkins that he might perform the operation. For this purpose the patient, who had been lying on his side on the table, was turned a little more on his face, or at least it was attempted to turn him, when he burst out into a kind of hysterical excitement, laughed loudly, and was with difficulty held on the table. Nothing was done during this excitement, which lasted about a minute. After it had subsided, I administered a little more amylene, although the patient had not recovered his consciousness; and then Mr. Hawkins performed the operation, which I believe did not last more than two minutes altogether. During the operation the patient was turned on his face. He rested, I think, chiefly on his knees and elbows. He was muttering in an incoherent manner, and making slight attempts to move, but was easily restrained. I gave him an inspiration or two of amylene now and then during the operation, with the intention of preventing his waking prematurely; for this purpose I turned the head a little to one side, and raised the face a little from the table.[1] I had concluded that the patient would not require any more amylene, and was expecting that he would show signs of returning consciousness or sensibility almost as soon as Mr. Hawkins had tied the suture which he was introducing; but, instead of this, the limbs became relaxed, and the breathing, though free enough, took on a noisy, snoring character.

This is a state which is common enough in the use of chloroform, and excites no alarm whatever, but I felt that it ought not to occur in the use of amylene, especially after it was left off. I therefore sought again for the pulse at the wrist, and could perceive it only with difficulty, if at all. I spoke to Mr.

[1] Less than an ounce of amylene was poured out, and it was not all used.

Hawkins, and we immediately turned the patient on his back. His face had already become livid, and his breathing was of a gasping character. Mouth to mouth insufflation of the lungs was performed, and between the insufflations there were spontaneous acts of inspiration, during which the air seemed to enter the lungs freely. In a minute or two the lips became of a proper colour, and the countenance had altogether such a natural aspect that the patient seemed to be recovering. The pulse at the wrist, however, could not be felt. No one listened to the chest at this time, for fear of interrupting the process of artificial respiration. After two or three minutes, Dr. Marshall Hall's method of artificial breathing was substituted for the insufflation, and it was continued very perfectly by the House Surgeons and others for an hour and a half, with the exception of two short intermissions, which will be mentioned. During three-quarters of an hour of this time there were spontaneous inspirations, during which air entered the lungs, in addition to that which entered during the turning process. Twenty minutes after the accident, the process of artificial respiration was suspended for about a quarter of a minute, to enable me to listen to the chest. I thought I could hear the heart beating regularly, but very feebly, and certainly there was a good vesicular murmur, and the air seemed to enter the lungs by the patient's own breathing, almost as freely as in health. At the end of three-quarters of an hour, with the permission of Mr. Hawkins, I introduced two harelip pins which had been connected with the electro-magnetic battery, with the intention of performing galvano-puncture of the heart. The needles were introduced to the depth of about an inch and a half between the cartilages of the ribs, just to the left of the sternum, and on a level with the nipple. They were afterwards found to have penetrated the walls of the left ventricle, near the septum, but without reaching the cavity. There was a quivering contraction of the pectoral muscle when the needles were first applied, but no effect on the heart. The needles ought probably to have been coated with some non-conducting substance almost as far as their points. There were no further efforts of inspiration after this time, but this was probably only a coincidence. The electro-magnetic battery had been applied in the early part of the treatment by means of the wet sponges applied to each side of the chest, but it produced no effect.

An examination of the body was made by Mr. Holmes, the Curator of the Hospital Museum, on the following day. A good deal of dark-coloured fluid blood flowed from the right cavities of the heart, and the left cavities contained but little blood. The heart was pale and somewhat friable; but a microscopic examination by Mr. Holmes did not show any fatty degeneration. The lungs were moderately vascular, and contained some small epithelial tumours of the same character as those removed from the back. There was a large cyst in one kidney, but, with these exceptions, the organs were healthy. The vessels of the brain were not distended, and that organ was altogether less vascular than is usual after sudden death. No smell of amylene was perceived in the body."

The accident in this case, Mr. Snow thinks, clearly commenced in the heart; the brain, he says, was never more than partially under the influence of the amylene; its fatal action was undoubtedly exerted on the nerves of the heart.

59. *Trial of Miss Madeleine Smith.*—The case of Madeleine Smith is in every aspect of it the most extraordinary in the annals of criminal judicature. Not even extending our review to an age so remote as that of the Marchioness de Brinvilliers do we recall a case presenting, on the hypothesis of guilt, a mystery of human nature so astounding, or upon the hypothesis of innocence, a problem of circumstantial evidence so perplexing. A young lady, while yet a girl—not yet 20 years of age—was supposed to have ruthlessly poisoned her lover; not for forsaking her—that would have been comparatively a common case—but to get rid of him, in fact, for loving her "not wisely, but too well." Women have often slain their lovers for infidelity; but this would have been the first recorded case in which the gentle heart of woman so steeled itself against affection as to be capable of murdering an ardent lover, merely in order to be relieved from his attachment.

Of course the theory of the prosecutor suggested a motive, to meet this moral difficulty. The suggestion was that she had sacrificed him to interest, and that she had got rid of him because he was in the way of her union with a richer lover. This would have been without much moral probability, for although, alas! *men* have often sacrificed affection to expediency, such is not the nature of woman. But the suggestion was involved in this difficulty; that the supposed victim was the favoured lover; that the other had been previously known by the young lady; that she had preferred the secret rival; and that her acceptance of the other had rather been in deference to the wishes of her parents. So that the suggested solution of the moral difficulty only raised a greater; for we were to suppose a girl murdering her favourite lover in order to facilitate her union with one to whom she had preferred him, but her anxiety for her marriage with whom was supposed to be so great, that, though it had not been sufficient to prevent her from falling in love with another, it was powerful enough to induce her to murder that other and favoured lover!

To meet this difficulty another theory was suggested—that she murdered him from fear of exposure. Having, it was urged, at last accepted the first known lover, and having had secret interviews and clandestine correspondence with the other, she was anxious to prevent that exposure which his jealousy had threatened. And this theory it was sought to support by her own confession, that she had sent for him to persuade him to give up her letters. But then, again, this theory was also fraught with difficulties greater than that which it was proposed to meet; for as her letters were in his possession, and she had no access to his residence, and had never been there, and had no acquaintance with any of his connections, on his death they must infallibly, as, in fact, they did, fall into the hands of strangers. Moreover, if the intimacy had stopped short of what was criminal, the utmost that could be exposed was a foolish love affair, which a fond parent or an attached lover might easily pardon; and if it had gone to the extent of what was criminal, then there was a probable contingency in the event of which his death would have been her utter ruin.

These were the anterior moral difficulties raised by the theory of guilt. The evidence raised other improbabilities quite as puzzling. The theory of the prosecution was that on several previous occasions she had administered poison to her lover, and in this extraordinary way: their meetings were secret, in her father's house (or rather at her father's door), at the dead of night; and she is supposed to have given him once or twice before the fatal occasion a cup of cocoa drugged with arsenic. It is supposed, further, that he suffered severely on these occasions—that he suspected the draughts he had received from her—that he had even expressed his suspicions to a third party—and that, notwithstanding all this, he went again to a stolen interview, drank again from the drugged, and this time the fatal, cup, and fell a victim to his fond infatuation. Surely if this were so, the case is, in that aspect alone, without a parallel. That a man should, almost knowingly, take poison from the hands of his mistress, involves, indeed, an infatuation which could hardly have been conceived by the wildest imagination. She purchased poison; she was in a state of frenzy; it could only have been for her lover. Such was the case for the prosecution. But do girls in frenzy never poison themselves? Her letters certainly show this to have been more probable than murder.

The surrounding circumstances of what is supposed to have been the actual deed of murder raised other difficulties in detail quite as insuperable. It was proved that on the 4th of March she had written to him, advising him to go away to the south of England. The poison she purchased (as it is suggested) for this last and fatal draught was bought on the 6th, the very day she went away herself from Glasgow for a week or two. On her return he went away, and was away on the 21st March. The night of Sunday, the 22d March, was the fatal time. On the Friday she did not know he was away, and wrote, making an appointment for the Saturday night. That appointment, which was couched in the most endearing terms, he did not and could not keep, because, being away from Glasgow, he did not get it until the Sunday. The whole strength of the case for the prosecution on this, the very gist of the case, was rested on the fact, that nevertheless, he at once proceeded to Glasgow, as it is

suggested, for the purpose of visiting his lover, and that he was seen near her house that fatal night, but as early as nine o'clock; and then, as the evidence for the prosecution showed, he was in quest of some one else, on whom he actually called, and whom he much wished to see. But the night constable, who had seen him on former occasions near to, and at the house of his lover's father, did not see him there on that night. So that while, on the one hand, he was not proved to have been at her house on that night, his being in the neighbourhood was otherwise and naturally accounted for. Nor was this all. He had not kept the appointment conveyed to him for Saturday. She did not know the reason—that he was away from home—and therefore there was nothing to lead her to expect him on the Sunday night. On that night she went to bed about eleven o'clock (when he was seen in the vicinity it was two hours before)—and with a younger sister who slept with her. There the evidence left her, and left him, for several hours.

At half-past two on that fatal night or morning, he staggered to his door, labouring under the first effects of poisoning by arsenic; for eight hours he suffered agonies, during which he never alluded to any administration of poison, and after his death he was found to have received some quantity of arsenic, which could hardly have been less than 160 grains, for upwards of 80 were actually found in his stomach, and might have been even more than that quantity; three or four grains being sufficient to destroy life.

The theory for the prosecution was, that in that interval, between eleven and half-past two, the whole of the dread tragedy of crime had been acted; that the lover had come to the door of his beloved; that he had managed to awaken her without disturbing the family: or that she, without knowing that he was coming, had craftily risen for him so soon as her sister slept; that she had then prepared the deadly draught, boiling the arsenic with the cocoa—(for the chemists said that would be necessary to absorb so large a quantity without certainty of detection by the party drinking it), and that then she had induced her lover, for the third or at least the second time, to take from her hands, at that unseasonable hour, under such suspicious circumstances, the same sort of draught which he had before suspected to be poison; and which he could hardly have avoided suspecting to be poison again! And that then, having given him the fatal dose, she had conversed with him until the first effects began to display themselves; had then dismissed him with the usual blandishments to go and writhe a few hours in quiet and unaccusing agony, and die; and that she then again undressed and went to bed with her young sister, who found her next morning there as usual, and with calm and tranquil countenance accompanied her to the family breakfast table!

Surely such a combination of improbabilities—of almost impossibilities—required stringent proof; and the Lord Advocate declared that in this case "there was not a link wanting in the evidence;" that "there was no loophole at which the unhappy prisoner could escape." Such was certainly not the opinion of the legal profession in this country, nor, as we are informed, in Scotland. And that was rather reckless language to be used by the first law officer of the Crown, in a case of life and death; it was language which should hardly be used if the case was clear and cogent. But was this such a case? The case of a girl, accused of poisoning her lover—a man who was not proved to have been with her on the fatal occasion! Circumstantial evidence must often be resorted to, and inferences must be drawn from facts. But the inference of guilt must arise from the facts proved. And here, even if the man had been seen at the house on that night—the reception of the poison could only have been an inference. Then, however, it would have been an inference from a fact; here it was sought to make an inference upon an inference—nay, rather against the facts. For the evidence rather tended to show that he was not there on that night—at any such hour as is supposed; or that if he were, she could not have seen him. Was this a case to characterize as one in which there was not "a link wanting," and in which there was "no loophole for escape?" We rejoice that the jury thought otherwise.

The Scottish law differs from ours in several points. In one, which presses most cruelly upon the accused; in another, which is rather in his favour.

The accused is liable to be examined previous to commitment, whereas in England the voluntary confession of a prisoner is alone received.

In this case the first, if not the only information, on which the Crown constructed their whole case of a poisoning by arsenic, in the medium of cocoa, was derived from the answers of the prisoner (which were declared to have been singularly frank) to the questions, whether she had ever given him anything to take at their interviews? and whether she had ever purchased arsenic? There were some irregularities—observed upon by the Court—in the manner of taking the evidence of the only witness (Miss PERRY), who at all connected arsenic with the administration of cocoa by the prisoner; and she stated that the deceased had complained to her that he had been ill after he took cocoa from "her," which was explained to mean the accused, although he did not name her. Now as this was the solitary piece of evidence which bore at all strongly on the accused, and as it appeared that on one point—an important point of date—Miss Perry was enabled to correct her evidence by a suggestion from the official examining her, we should like to know whether, when she made that statement as to the observation of deceased about cocoa received from "her," Miss Perry had been informed or allowed to gather that the accused had acknowledged the administration of cocoa, and the purchase of arsenic! If so, then—as we all know how quickly, especially under excitement, the mind will weave inferences to mislead the memory—probably the testimony of Miss Perry, which told so terribly against the accused, might have been a mere error; and, anyhow, it illustrates the fearful peril which attends the practice of a compulsory examination of an accused party.

The other point of Scotch law to which we refer is, that the jury are not bound to find the prisoner "guilty" or "not guilty," but they may simply say "not proven." At first sight this might seem an advantage to the accused party, as compared with the English system, but perhaps practically it is little, if any, for the English jury must be unanimous; which always secures the accused the benefit of a doubt. On the other hand, the Scottish verdict "not proven" leaves a painful doubt on the character of the accused; and we are surprised that in this case the verdict of "Not Guilty" was not recorded on all the counts, for we cannot help thinking that the hypothesis of guilt is encompassed with difficulties actually insuperable; and so widely do we differ with the Lord Advocate, that so far from thinking that there was no link wanting in the evidence, we really do not see any link in it to connect the accused with the catastrophe, except that "link" which was supplied by the evidence of Miss Perry, the whole strength of which was destroyed by the strictures the Court passed upon the mode of her examination, and which in another point of view told in favour of the prisoner.

It is indeed impossible not to see that (as we said at the outset) the hypothesis of Miss Smith's innocence involves a mystery. But then it is a mystery not necessarily associated with suspicion as to her. For the remarks we have made (all founded on the evidence for the prosecution) go to show not only that she was not proved to have done the dreadful deed, but that it is in the highest degree *à priori* improbable, and, on the evidence, all but impossible that she could have done it. And on the hypothesis that she did not do it, it is not of course for her to show who did it—whether the deceased or some other party. On that head it is enough to say that even the evidence for the prosecution left it, to say the least, quite as probable that he poisoned himself, as that the accused poisoned him; and while the evidence for the defence accounts for her purchases of poison, it raised a great probability that he also was in possession of arsenic. Upon the other hand, the character of the man, and the admitted fact that he was in correspondence, and on intimate terms with other women, leaves a pretty large loophole (to use the phrase of the Lord Advocate) for a supposition that poison might have been administered by some other person than the accused. At all events, the evidence is consistent with the hypothesis that some other party caused the death; a state of things in which, according to the doctrine of all writers on criminal jurisprudence, the accused is entitled to an acquittal.—*Evening Herald.*

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

On Starch as a Product of the Liver, and on the Amyloid Degeneration of the Liver in Yellow Fever. By SAMUEL JACKSON, M. D., Professor of the Institutes of Medicine in the University of Pennsylvania.

The existence of an amyloid substance was demonstrated by Virchow and Bennett, to be the peculiar anatomical alteration of the liver, in many cases designated as waxy or fatty. In alluding to this fact in my course of 1855, the suggestion was made that the peculiar appearance of the yellow fever liver, supposed to be a fatty degeneration, might possibly be owing to a similar modification. The attention of the southern gentleman who would possess the best opportunities of making the requisite observations, was invited to this subject as an investigation of great interest. In October of that year I had made some experiments on slices of liver, sent to me at my request, from the Pennsylvania Hospital, taken from a patient who had died of yellow fever. The quantity was too small for complete examination, and the commencing alteration of the specimens rendered the results imperfect.

This prevision has been, to a certain extent, verified by my young friend Professor Joseph Jones, of the Savannah Medical College, as will be seen in the following extract from a letter received from him of date June 24.

"On the first of next month (July,) I expect to take charge of the Savannah Hospital in conjunction with Dr. Arnold. My object is to investigate the physical and chemical changes of the solids and fluids in our southern bilious, remittent and yellow fevers.

"Last summer I had the opportunity of examining several yellow fever livers chemically. Their reactions differed from those of bilious and remittent fevers. They contained a substance which gave reactions similar in all respects to cellulose, and presented under the microscope an appearance like the granules of starch. The reaction of the bilious fever livers also showed the presence of cellulose, but in much smaller amount.

"A solution of potassa readily dissolves the liver of bilious fever, but acts very slowly on the yellow fever liver.

"I endeavoured to make gun cotton out of yellow fever liver. If I had taken sufficient care, it is probable I might have succeeded. My mind, however, is not at all satisfied with these partial results, and I have not therefore given them any publicity."

It appears to me to be no more than justice to Professor Jones to make the above statement, as the subsequent researches of M. Cl. Bernard fully sustain his observations, while he has been anticipated by M. Pelouze. In consequence of his laudable desire to present his investigations in a more complete and authentic form, he has lost precedence in publication.

M. Cl. Bernard, in following up his important discovery of the sugar-making function of the liver, had satisfied himself "that sugar was not an immediate product of the liver derived from any element of the blood; but that it was constantly preceded by the creation of a special matter capable of generating sugar by a sort of secondary fermentation."

It became necessary, before further progress could be made, that this spe-

cial matter should be isolated, and its nature and properties be ascertained. M. Bernard has succeeded in accomplishing this important chemical fact, by a most ingenious and admirably devised process and manipulation. The details of this process were communicated to the *Académie des Sciences*, March 23d of this year. At a subsequent meeting of the *Académie*, M. Bernard communicated verbally a more simple and direct method for obtaining liver starch than the one he had originally employed.

The glucogenic or sugar forming matter of the liver, obtained separately, possesses all the characteristics of hydrated starch. The one of most interest, is that relative to its change into sugar. "All the influences without a single exception which transform vegetable starch into dextrin and glucose, equally change the glucogenic matter of the liver into sugar passing through the intermediate state of dextrin."

From this important discovery by M. Cl. Bernard, it results that the liver sugar is generated by two distinct acts, the one vital or physiological; the other chemical. The liver starch appears to be produced by the act of nutrition or the proper organic actions; while this substance is transformed subsequently into sugar under the influence of a special ferment, that is brought to act on it in the liver.

At first M. Cl. Bernard supposed that this ferment was special to the liver like the glucogenic matter. He even succeeded in obtaining it in an isolated state, though he gives no account of it. But he soon found that the blood generally possessed this fermentative power, and transformed the sugar making matter with very great energy into sugar. The ferment of the liver is, then, evidently derived from the blood circulating in it.

These highly interesting researches of M. Cl. Bernard, establish in apparently the clearest manner, that in the normal state, the liver, by its nutritive or proper organic actions, creates an amyloid substance, analogous to, if not identical with, vegetable starch; and that some element of the blood, acting as a ferment, transforms it into glucose or hepatic sugar with a rapidity nearly as great as its formation.

M. E. Pelouze took up the chemical history of the glucogenic substance of the liver where it was left by M. Bernard, and has completed it. He has proved the correctness of Professor Jones' idea, that gun-cotton might be made from the liver. "I sought," he observes, "whether the (hepatic) glucogenic matter, under the influence of fuming nitric acid could be transformed into xyloïdine like starch." The results were identical. "The xyloïdine obtained was very combustible and detonated with flame when heated to a temperature of 180°."

Under the influence of diluted nitric acid, it was converted into oxalic acid.

Analysis furnished the following result :—

Carbon	39.8
Hydrogen	6.1
Oxygen	54.1
	100.

corresponding to the formula

$$C_{12}H_{12}O_{12}.$$

The formula of vegetable starch, treated in the same manner as hepatic starch, is

$$C_{12}H_{11}O_{11}.$$[1]

[1] Comptes Rendus, 29 Juin, 1857, p. 1321.

The identity of the two is fairly established, and the function of the liver is not glucogenic, but amylogenic.

Assuming as correct the discovery of M. Cl. Bernard, that starch and not sugar, is the immediate product of the liver, the mechanism or process of the anatomical modification of the liver in yellow fever, becomes apparent. The yellow fever poison introduced into the blood, vitiates its vital, anatomical, chemical and physical characters, properties, relations and modes of action in the organism. All its functions are at first perverted, and ultimately, if the dose be powerful, are suspended or destroyed. The blood ceases to be normal. This profound change of the circulating fluid, the effect of a large dose of the poison, is invariably fatal.

The fermentative power of the blood being more or less impaired, and finally destroyed, the transformation of the amyloid substance of the liver into sugar, is imperfect, or is not accomplished; the secretion of bile is at the same time arrested, and in consequence the amyloid element of the liver assumes an undue proportion in its structural composition.

The peculiar character of the liver in yellow fever was first distinctly announced by M. Louis, as observed by him at Gibraltar. It has been usually supposed to be a form of fatty degeneration, though no regular and sustained chemical analysis, that I am aware of, ever demonstrated the fact.

The theoretical deduction from Bernard's physiological facts, would demonstrate it to be an amyloid degeneration; and this theoretical conclusion is borne out by Professor Jones' observations on the yellow fever liver. What gives great weight to his facts is, that his results were anterior to Bernard's discovery of the amylogenetic function of the liver. He was under no preconception that often deludes observation. On the contrary, he appears to have been staggered by the unexpected discovery he had made, and postponed announcing the fact until he had fortified his results with additional observations.

Professor Jones found unequivocal evidences of cellulose (amyloid substance) in a liver of a patient who died of bilious remittent. This is to be expected, for if the production of an amyloid matter is a normal function of the liver, it will be found in most cases of acute disease, in a certain proportion.

It may be questioned whether Louis' yellow fever liver will prove to be an invariable attendant on yellow fever. In the epidemic of 1820, I made a *post-mortem* examination of a man, who died in less than 24 hours from the attack, in consequence of violent cerebral symptoms, attended with convulsions. The stomach was filled with black vomit. The liver was of a deep purple hue, gorged with fluid blood that flowed in copious streams from incisions, and was collected in several cups for the purpose of examination. The characters of yellow fever liver are generally developed in proportion to the duration of the case.

In a letter of date August 13th, Professor Jones informs me he had had an opportunity of examining a true fatty liver, and could detect no signs of cellulose (amyloid matter) in it.

The preceding facts authorize the following conclusions:—

1. That the liver is a generator or creator of amylum or starch (Bernard and Pelouze).

2. That the hepatic starch is transformed in the liver by a ferment existing in the blood into dextrin and glucose, in the same manner as starch in vegetables is transformed by diastase into dextrin and glucose (Bernard).

3. That in yellow fever from the alteration of the blood this transformation

does not occur, and that an amyloid substance, probably cellulose, exists in the liver to a large amount (Jones).

4. That this anatomical alteration of the liver is not special to or pathognomonic of yellow fever, but occurs in other fevers (Jones), and in some chronic forms of disease (Virchow, Bennett, and Carter of Ed.).

5. That this pathological alteration of the liver has no direct influence on the rapid course of yellow fever and its great fatality, which are due solely to the immediate power of a specific poison corrupting the blood and extinguishing the vital forces.

Vaccine Virus administered Internally for the Cure of Smallpox.—[We have been favoured with the following communication, and cheerfully lay it before the profession. Though the new remedy may be regarded with some suspicion, it is only by trial that its power can be positively determined.]

DEPARTMENT OF STATE, WASHINGTON, *July* 28, 1857.

TO DOCTOR ISAAC HAYS, *Editor of the American Journal of Med. Science, Phila.*

SIR: I inclose herewith a copy of a communication recently transmitted to this Department by N. O. Cornwall, Esq., Consul of the United States at Rio Grande do Sul, in Brazil, in relation to the alleged efficacy of the vaccine virus administered internally for the cure of smallpox.

It does not fall within the province of this department to express any opinion as to the value of the discovery claimed by Doctor Landell. I have thought it due, however, to the cause of humanity, and to the spirit of philosophic investigation which characterizes the science of medicine, to place Doctor Landell's claims before the profession, in his own words, in order that they may undergo that ordeal which will be the true test of their value; and that, also, by their confirmation, the name of the discoverer may be written among the benefactors of the human family, imperishable as that of Jenner, and forever associated with it.

It may be proper to mention that an extract from Dr. Landell's memoir has been communicated to the *Union* newspaper of this city. Your valuable journal has been selected as the representative of its class, in order to bring the subject directly to the attention of the profession. It would be a matter of infinite satisfaction to know that the anticipated results of Doctor Landell's mode of treatment have been verified. I am, sir,

Your obedient servant,

LEW. CASS.

Vaccine given Inwardly for the Cure of Smallpox. By R. LANDELL, M. D.—In the months of August and September, in the year 1837, in this city, a terrible epidemic pestilence of the smallpox cut off many valuable lives. I also had to lament the loss of a very fine boy, who fell a victim to this contagious epidemy. The sorrowful remembrance of this event induced me, when the disease broke out again in the year 1842, to put in practice an idea which I had since that unlucky year, 1837. My opinion was that, by giving inwardly vaccine lymph to those sick of the smallpox, it would be rendered much milder. I also requested my son, John Landell, now Doctor in Medicine, to make some trials under my directions of vaccine internally, in various of my patients labouring under smallpox; the results were the most satisfactory, because they became benign and distinct. So, then, counting from this year, 1842, till the year 1854, and again until these last months of the present year, I have verified the virtue and advantage which are to be derived from the internal use of vaccine for the cure of variola or smallpox. Based on these principles, I perform a humane duty in making known to the public this system, which I have adopted for the cure of smallpox.

Dissolve the vaccine that is contained on a pair of plates or a capillary tube, which is about four or six drops of vaccine lymph, in four or six ounces of cold water, and give to the patient a tablespoonful every two or three hours.

The favourable result of this exhibition is that it mitigates the symptoms, modifies the species, and cures the smallpox.

I recognize that, as vaccine applied externally prevents the smallpox, so also being taken inwardly, in the manner above indicated, it cures quickly and efficaciously the smallpox in all its stages.

Under its use, the fever, the delirium, the hoarseness, diarrhœa, pneumonia, cerebral congestion, and finally the secondary fever, disappear.

Beginning the treatment on the second or third day of the eruption, the smallpox becomes as varicella or varioloid, although the epiderm is thickened and in a state of congestion, in five days dry without suppuration.

Applying the same treatment on the fourth or fifth day of the eruption, the smallpox become as if they were the *true vaccine*, fill and dry in the space of ten days, with suppuration.

Considering, then, that the vesicles and pustules ought to be opened for two or three times, always when they contain any liquid, and beginning the third day to prevent the secondary fever. I have had since 1842 more than thirty cases, and in fourteen paid particular attention; they were three severe confluent cases, and eleven less severe, although distinct.

Since I had recourse to this treatment I have not lost a single patient of the smallpox. At my request, some of my colleagues are using this system, and they, as well as I, have reaped the most flattering results. These effects are superior to my expectation, and even to my comprehension; in fact, the vaccine neutralizes the variolic virus, or one morbid action destroys the effects of another. By this treatment I have seen disappear the fever, delirium, hoarseness, diarrhœa, pneumonia, cerebral congestion, and the secondary fever.

It may be mentioned here that the use of emollient clysters or castor oil internally, to keep the bowels loose, and in children calomel, is very necessary; as also gargles of nitrate of silver and chloruret of lime. And after the fifth day, give baths of warm water, with a little chloruret of lime or chloruret of soda, or sponge the body.

Also have given vaccine inwardly as a therapeutic remedy in hooping-cough, and with benefit; in some cases the hoop or convulsion cough disappeared in ten hours, remaining only a simple cough, which extinguishes in four or twelve days.

For my part, I render thanks to God for giving me the knowledge of this method to cure by so admirable a mode a disease so Satanic as the smallpox. Would to God that this small contingent of my poor intellect serve to assuage the sufferings of those labouring under this cruel epidemy!

PORTE ALEGRE [BRAZIL], *March* 12, 1857.

Case of Fracture connecting with Frontal Sinus and Exposure of Frontal Nerve. The Nerve divided. By W. LEHMAN WELLS, M. D., Resident in Pennsylvania Hospital.

John Funk, aged 44, teamster, a resident of Holmesburg, was admitted Monday, January 12th, 1857, at 5 A. M., with compound fracture of the frontal bone, which had occurred twelve hours previously. He had been employed in a livery stable at Holmesburg, and while in a stooping posture

was kicked over the right eye by a horse, and thus rendered insensible for some time.

There was a wound about three inches long just below the eyebrow, which extended also a little on each side. This had been sewed up at first, the man said, by a surgeon, but afterwards opened, with the exception of a stitch near each corner, on account of hemorrhage. The neighbouring parts were swelled very much, and there was ecchymosis around both eyes, which on the right side extended under the conjunctiva, and was very great The bleeding had not yet altogether stopped, and must have been considerable from the appearance of the bandage and his clothes. It, however, soon ceased spontaneously upon removing the bandage, turning out the clot, and sponging with cold water.

There had been also bleeding from the nose, which was accounted for by a depressed fracture of the orbital plate of the frontal bone. This fracture was just internal to the frontal nerve, which was exposed in one or two lines of its length. There was very great pain both in the eye and in the forehead.

Fearing that the danger of inflammation from the injury to the eye would be increased by reflex action from the exposure and laceration of the frontal nerve, I divided it as closely to the proximal extremity as possible, causing very great pain for a moment, and then immediate relief from all the pain he had previously had in the eye, and nearly all on the forehead. The edges of the wound were brought together by the interrupted suture and adhesive plaster, and lint wet with cold water applied. He took sennæ fol. ʒj, magnesiæ sulph. ʒij, which operated twice.

In the afternoon, his pulse being 90, skin warm, and tongue furred, he was ordered liq. potassæ citratis fʒss every three hours. This was stopped the next day, as all symptoms of fever had subsided.

The wound healed up rapidly, except in a small place, where a sinus remained with a piece of dead bone at the bottom. Before this became loose, however, he went away, at his own repeated request, on the 2d of February, having remained twenty-one days. His sight, after the first day or two, was perfect on both sides.

Extrusion of an Ovum at the Fifth Month of Utero Gestation with the Membranes entire. By J. F. GRANT, M. D., of Bradshaw, Tenn.—I was called to Mrs. P. with hemorrhage, together with other symptoms of abortion, consequent upon an injury which she had sustained. She was five months gone in pregnancy. Shortly after my arrival, a fœtus was extruded with the placenta and membranes entire. The circulation went on beautifully for thirty minutes, when I severed the cord, which was still pulsating regular and with good volume. I think it is probable that the circulation might have been kept up for several hours and perhaps days. This and other similar cases that have occurred certainly go very far to prove the independent existence of the fœtus in utero, bearing the same relation as the chick in ovo does to the parent bird. It was a case of twins, but the latter was expelled after the rupture of the membranes.

P. S. The child died immediately after the cord was cut.

Indian Corn in Intermittent Fever. By D. B. PHILLIPS, M. D., Passed Assistant Surgeon U. S. N.—I beg leave, through the medium of your journal, to call attention to the efficacy of Indian corn (maize) as a specific of some value in intermittent fever.

Having been called to a case of this fever in the country, and having no medicine convenient, I determined to make trial of a negro remedy, maize. My patient had been suffering with the quotidian type of intermittent fever several days before I saw him. On Wednesday, at 11 A. M., when I was first called to see him, he was in the hot stage, and so high was his fever that he was occasionally delirious in a slight degree. I commenced by giving him a large tablespoonful of uncooked and unsifted corn meal, mixed in a glass of cold water, and directed him to take the same quantity every two hours. At night I gave him six grains of calomel (the only medicine I had). On the following day, Thursday, the same treatment was continued (with exception of the calomel, which was given but once), and the paroxysm, which was much milder than on Wednesday, was postponed until 1 P. M. I continued the meal and water for two days more, during which there was no return of the disease. I then discontinued it altogether, and the patient has been well and hearty ever since.

Of the *modus operandi* of maize I know and can say nothing. My patient remarked that it relieved his thirst in the hot stage more effectually than anything he had ever used before, and the fever was more rapidly subdued by it. I trust that future experience may confirm the virtues and value of this simple remedy.

WASHINGTON, D. C., *July* 15, 1857.

Account of a Case in which both Ovaries were extirpated by Dr. H. A. Potter. By JAMES H. TINKHAM, M. D., of Geneva, N. Y.

Inclosed is an account of an operation in which both ovaries were successfully removed by Dr. H. A. Potter, of this place, and also a brief history of the case. As it is an operation of the first magnitude, and one which presents some points which may be of importance when the subject of ovariotomy is considered, I have no doubt it will prove interesting to your readers.

The patient was a married lady, twenty-five years of age. She first discovered a small tumour in abdomen about two years ago; from that time to the present it has increased gradually, but within the last two months more rapidly than before. During this time her general health has been good, she has been under no medical treatment, nor has the tumour been tapped. When she consulted Dr. Potter the tumour was of immense size, distending the walls of the abdomen from the pubis to the sternum. He decided, from its appearance and previous history, that it was an enlarged ovarium, and ordered it to be tapped. This was done on the same day, and nine pounds of a dark coloured, pasty-looking fluid drawn off. Having now ascertained beyond a doubt the character of the tumour, he recommended an immediate operation as affording the only chance for recovery. This, having been decided on by the patient and friends, was performed in the following manner by Dr. Potter, assisted by Drs. Peck, Bolton, Graves, Whitbeck, Hawley, Frost, and myself. The room was heated to a temperature of about 80°, the patient placed on a table of convenient height in the centre of the room, and chloroform administered. An incision was then made extending from the umbilicus to the symphysis pubis along the linea alba, the tissues were carefully dissected until the peritoneum was reached, some small arteries were ligated, the blood sponged away, and the peritoneum opened, when the tumour presented itself. It was slightly adherent to the omentum, but was easily separated from it by the fingers and handle of the scalpel. A trocar was introduced into the lower part of the tumour, and a large quantity of fluid nearly

the colour of water drawn off. As this did not empty the tumour, it was evident there were two cavities. The trocar being introduced into the other cavity, about the same amount of fluid was evacuated as before, but of a darker colour and thicker. As the size of the tumour was still such as to prevent its being lifted from the abdomen, the incision was extended above the umbilicus about two inches; the tumour was then easily lifted out, and proved to be the right ovarium, the broad ligament of the uterus constituting its pedicle. This pedicle was cut close by the tumour, was split longitudinally into three parts, and a strong ligature applied firmly to each part. This tumour with its contents weighed twenty-two pounds. The left ovarium was then examined. It was about the size of a large hen's egg, and in a diseased condition; it was consequently removed, and a single ligature applied to its pedicle. The protruded intestines were placed back with linen cloths wet in warm water. The lips of the wound were brought together and retained by the common interrupted suture and adhesive straps. The cut extremity of the pedicle was placed as near as possible to the lowest part of the wound, and retained in place by three ligatures, which were fastened together and brought out between the lowest suture and the lower angle of the incision. A strip of lint and a thick cotton compress were laid on the wound, and the corset bandage applied—a bandage well adapted to prevent danger from action of abdominal muscles, and one which could be easily opened. The patient was left in charge of Dr. Whitbeck, attending physician, from whose report of the after-treatment I extract the following portions.

April 23*d*. Four hours after operation, patient felt considerable pain in lower part of abdomen; pulse from 110 to 115. Catheter introduced, and a quantity of urine removed. Prescribed sul. morphia ¼ gr.

24*th*. Pulse 125; tongue dry; pain in abdomen. Evacuated bladder by means of catheter. Gave small doses of veratrum viride at intervals of four hours, which reduced frequency of pulse from 125 to 98, at or near which point it has remained ever since.

26*th*. Patient seized with vomiting in morning. Gave a teaspoonful of brandy in a little water, and afterwards small doses of sul. morphia. Bad symptoms soon subsided. Also gave submurias hyd. ij grains every two hours, until ten grains were taken, followed by castor oil and an enema. A free evacuation of the bowels took place during the night.

The rest of the treatment consisted in an occasional dose of morphia and of veratrum viride. The ligatures were all removed by the 25th of May, and the patient discharged.

—

Case of Herpes Preputialis. By H. T. Goldsborough, M. D., of Easton, Md.—In looking over the *American Journal* for July, 1854, I find a case of "herpes preputialis," which had returned after being cured five times consecutively.

A case of precisely similar character was cured by me in the following manner: The disease occurred in a young and healthy man, without previous exposure. The herpes yielded readily to the usual astringent remedies, but appeared to *lie dormant* in the glands and prepuce, and recurred upon disorder of the stomach, or the presence of fever in the system. Attributing, finally, the recurrence to the heat and secretion of the glans (cleanliness being all the time strictly enjoined and observed), I directed that the prepuce should be kept retracted behind the "*corona*." This was attended at first

with some inconvenience, but by perseverance the prepuce adapted itself to the retracted position, the secretion was diminished almost entirely, and I am glad to say that the herpes has not since returned.

I have been led to relate this case simply because I have not seen the same mode of treatment adopted elsewhere.

July 12, 1857.

DOMESTIC SUMMARY.

Dislocation of the Processes of the Cervical Vertebræ.—Dr. E. R. Maxson reports (*Buffalo Med. Journ.*, Jan. 1857) an interesting case of this in a girl nine years of age, who, when playing, after a sudden turn of the face toward the left shoulder, experienced pain in the back of the neck, became faint and was unable to turn her head back to its natural position. She continued much in this condition until late in the evening of the second day, when in attempting to move her in bed the face, by accident, was turned a little more toward the left shoulder, as was supposed, when she was severely convulsed for a considerable time: after which she became very faint, and could not bear to be raised up much in bed. At this time, forty hours after the accident, Dr. M. was called to the patient. He found her in bed with her face turned to her left shoulder, and without the ability to move or turn the head. The eyes were apparently fixed, and the countenance unnatural. On passing his hand along the spinous processes of the cervical vertebræ, Dr. M. distinctly felt at the fifth and sixth vertebræ an irregularity of the spinous process of the fifth or sixth vertebræ appearing to the right of the one below. Dr. M. feeling satisfied that there was a dislocation grasped the head with both hands and proceeded according to Desault's method, only he first carried or turned the face very gently a little further toward the left shoulder, to, if possible, disengage the process; then lifting or extending the head, he turned the face very gently toward the right shoulder, when the difficulty was at once overcome, and she exclaimed, 'I can move my eyes.'

"Her countenance soon acquired a more natural appearance; the faintness passed off; she rested quietly through the night; had no return of the difficulty, and needed only an emollient anodyne to soothe the irritation and slight swelling which remained at the point of injury.

Change of Colour in a Negro.—The following remarkable example of this is related by Dr. W. L. Sutton in his Report relating to the Registry and Returns of Births, Marriages, and Deaths in the State of Kentucky for 1855. "This year, 1855, there was born, at Mr. J. Smith's, in the county of Fayette, a *lusus naturæ*, which is very rarely found. In May, a female child was born, entirely black, and of black parents. When I saw it, in August, a large portion of the hands and forearms was of the appearance of the most delicate skin of a white child of the same age. There was little gradation of colour, but the change was abrupt. On other parts of the body were spots of a smaller size, and of the same colour. Since that time considerable changes have occurred. The same portions have been alternately white and black. I visited the child a few days ago, to see its present condition. The child was represented to have enjoyed good health, and at first sight it might not have been remarked as at all different from many negro children. The skin is generally black, of a rather dry aspect, as if she was not in very good health. There are no extensive coloured spots, but she is sprinkled all over the surface with spots from the size of a millet-seed to that of a grain of Indian corn, of irregular shape, but generally inclined to round or oval. These spots have nothing of the delicacy of appearance which characterized the white portions soon after birth; but were of a dull ashy colour. There is nothing in the colour or condition of the hair or eyes to distinguish her from other negro children.

"In the July, 1846, number of the *American Journal of Medical Sciences*, is an account of a somewhat similar change of colour observed in a native of Africa, by Thos. J. Savage, M. D. In that case the change had been preceded by serious indisposition. The health before and after that time was reported to have been good. The colour of the feet and ankles, in the plate furnished by Dr. Savage, gives a very good idea of the colour of the hands and forearms of the child above mentioned, but there was more delicacy of colour in the child; but at no time the same extent of surface undergoing change of colour; nor was there any desquamation of the cuticle. Unlike Dr. S.'s case, too, the parts exposed, and those covered are, at present at least, equally spotted."

Case of Death by Syncope from Plugging the Pulmonary Artery.—The following interesting example of this is recorded by Dr. O. M. Allaben, in the *Transactions* of the Medical Society of the State of New York, for 1857.

The patient, a male, 33 years of age, was attacked on the 4th of April, 1856, with inflammation of the lungs, for which, on the 8th, he was bled until complete syncope occurred, which took place much sooner than was anticipated, and before twelve ounces of blood were lost. Great relief followed the bleeding. He was directed a dose of calomel and jalap, to be followed by antimonial expectorants, and a blister to the chest. On the next day the urgency of the symptoms had considerably abated, but the patient still complained of some pain in the left breast, particularly over the region of the heart. The pulse was hard, strong and frequent. The patient was again bled with the same result as before, early fainting and abatement of symptoms. The other remedies continued.

The case progressed favourably until the 25th of April, when Dr. A. called to take leave of the patient. He found him free from symptoms of pulmonary disease, with a good appetite, and able to walk about the room. The pulse was, however, more active than the condition of the patient would seem to indicate. The patient complained 'of a sore spot at his heart.' From this, however, nothing serious was apprehended.

In the evening he proposed to join the family at tea. To Dr. A.'s objecting to his making so much exertion, he replied that he had dined with the family and would take tea with them. Accordingly, with the assistance of his wife he dressed himself, walked into the supper room and sat down at the table. Before commencing the meal he fainted. Dr. A. immediately inclined his chair backwards, elevating his feet above his head, and dashed cold water into his face. In about a minute afterwards he gasped for breath but exhibited no further sign of returning animation. A feeble pulsation was perceptible at the wrist. At intervals of about one minute each, the patient gave two successive gasps and expired. After the pulse had ceased at the wrist, and for some minutes subsequent to the last gasp, the action of the heart could be distinctly felt by the hand placed upon the chest. The body was examined forty-eight hours after death.

"Both lobes of the lungs presented a dark, uneven, mottled appearance, and had evidently been the seat of high inflammatory action. They had a spongy crepitous feel, and air bubbles escaped when portions were pressed between the thumb and finger, but no blood. The stomach, liver and intestines appeared healthy. The right side of the heart, including the vena cava, was found enormously distended with dark, grumous, coagulated blood. Upon breaking down the clot with the finger a reddish, organized, fleshy substance was detected, protruding into the coagulum from the mouth of the pulmonary artery, into which it had become firmly impacted, plugging it up like the cork of a bottle, and completely cutting off all communication between the heart and lungs. It was nearly as thick as the end of the little finger, and about an inch in length. The left cavities of the heart were of course found empty. The brain was not examined. The right ventricle of the heart had an unusually smooth and reddish appearance, and upon further examination there were found attached to its parietes, and filling up the sulci formed by the columni carnæ, two other organized deposits, similar to that taken from the pulmonary artery. One about the size of a small bean, and the other as large as that plugging the artery. Both had

a ragged, irregular form, and were attached to the heart by a vascular union, which easily yielded to a moderate traction, by which they were removed. These formations were preserved in alcohol, and after ten months present nearly the same appearance as when they were removed.

"The cause was now apparent. The organized substance found in the pulmonary artery, had been formed in the ventricular cavity, like those there found attached. By the constant action of the heart it had become detached, and by the very next pulsation was carried into the pulmonary artery, thereby cutting off the supply of blood to the lungs, left side of the heart, and brain, and causing immediate death by syncope. Whether these extraneous formations were the result of previous disease, or were produced by the same cause that caused the pneumonia, can never be known; but that inflammatory action of the parietes of the right ventricle had existed, admits of very little doubt; and that it had resulted in depositions of fibrous or coagulable lymph, seems equally certain. These had become organized by the extension into their substance of the excited vessels of the living surface, and they had become morbid growths. The unusual activity of the circulatory system during the pulmonary disease here finds its prompter; and the 'sore spot at the heart' points to the same exciting cause; while the great susceptibility to syncope may have been owing to the morbid irritability of the walls of the ventricle, and a consequent increased disposition to respond to the loss of its accustomed stimulus. The severity of the pulmonary symptoms for a while diverted attention from the cardiac difficulty; although my fears were frequently excited and as often expressed, that the heart was participating in the abnormal changes that were taking place."

—

Delirium Tremens following an attack of Pneumonia.—Dr. J. J. Summerville reports (*Transactions* of the Eighth Annual Meeting of the Medical Society of North Carolina) the following case:—

"J. C., a dealer in tobacco, from Patrick County, Va., aged 35 years, was attacked by pneumonia, May 5, 1854. This case presents nothing interesting in itself; and, but for the disease which made its appearance at the decline of the inflammation of the lungs, would not be mentioned as deserving particular attention. The treatment was that which I usually adopt in plain, uncomplicated cases of pneumonia, viz., tartar emetic in doses barely tolerated by the stomach during the day, with calomel and Dover's powder at night, vs., cups, etc.

"On the sixth day there were all the indications of approaching convalescence. On the seventh I found him up, and he stated to me that he was quite well, but that some one had been all the morning trying to shoot him through the window. The people with whom he was living told me that he had been walking about the yard that morning, and had seemed much annoyed and disgusted by the many loathsome objects which he fancied he saw on every side; nor would he be persuaded that these objects existed merely in his imagination.

"Here was a plain case of delirium tremens, and of course I made inquiry as to his habits of life. To my great surprise, I found that he was strictly temperate, seldom tasting spirits, and had never in his life been intoxicated. The usual treatment was instituted—brandy and opiates—until sleep was obtained. He continued restless and excited, with occasional paroxysms of terror, for about three days, when sleep overpowered him, and he recovered by degrees his right mind. Nothing unusual happened during his convalescence. He was much prostrated at first, but soon regained health and strength."

—

Poisoning from an overdose of the Tincture of Cantharides.—Dr. H. Kelly reports (*Transactions* of the Eighth Meeting of the Medical Society of North Carolina) the following case of this:—

"The subject was a mulatto child, two years and nine months old. Its mother, by direction of a physician, procured an ounce of tincture of cantharides for her own case. An elder brother of the subject, in the absence of the mother, got the phial, and persuaded the child to swallow a drachm—about two-thirds of the contents of the phial. In a short time the child became very sick and

restless, with frequent efforts to vomit. Two hours and a half afterwards almost incessant vomiting set in, and continued for some two hours, when it was discovered that the child had taken the poison. On my arrival, five hours after the accident, it threw up what the parents supposed to be a worm. This, however, I discovered to be the mucous membrane of the œsophagus entire, which I have preserved in alcohol. It was 4½ inches in length. I ordered a tablespoonful of flaxseed-tea to be given every 15 minutes, which was immediately rejected. Slippery elm was given, with the same result. At 8 o'clock P. M. I gave equal quantities of water and sweet milk, in the same dose, at intervals of 20 minutes. This was retained somewhat longer. At 1 o'clock A. M. it remained from three to five minutes before rejection. 4 o'clock A. M., no urine or feces had been discharged. An injection of flaxseed-tea was administered, and repeated within an hour, without bringing away any fecal matter. An injection of olive oil and molasses, a drachm of each, with four ounces of Castile soapsuds, was then given, by which a copious discharge of fecal matter was procured; a small quantity of urine was also passed, highly coloured, without any trace of blood. 2 o'clock P. M., patient better. Continue milk and water; every third dose flaxseed-tea or slippery elm. 10 o'clock P. M., patient retains drink for 10 to 15 minutes. Third day, 8 A. M., patient improving; same treatment continued; drink every hour; occasionally short naps of sleep obtained; passed a small quantity of urine, still high coloured. After injection, fecal discharges, without any appearance of blood. Drinks retained 15 to 20 minutes. 8 o'clock P. M., patient improving; drinks given alternately with rice-water; retained. Fourth day, 6 o'clock A. M., patient slept more during night; the fever, which has been high from beginning, is abating; emesis not more than once every two hours. 5 P. M., patient vomited but once since last visit; is clear of fever; urine assuming a natural appearance, but small in quantity; an injection was administered, which had the desired effect. From this time convalescence took place rapidly. In a week the patient was entirely recovered."

—

Extraordinary Obesity.—We find in the *Nashville Journ. of Med. and Surg.*, Aug., 1857, the following particulars respecting Miles Darden, whose size may appear almost fabulous, but the facts seem well vouched for.

"Mr. Darden was born in North Carolina in 1798, and died at his residence in Henderson County, Tennessee, January 23, 1857. He was seven feet six inches high. In 1845 he weighed eight hundred and seventy-one pounds, and at his death a fraction over *one thousand pounds*, and was unquestionably the largest man in the world, and since 'those days' in which there were giants, the largest man the world has produced. Up to 1853 'he was quite active and lively, and laboured,' after which, his fat increasing, he was compelled to stay at home or be hauled about in a two-horse wagon. It required thirteen and a half yards of flax cloth a yard wide to make him a coat. It required sixteen yards of cambric for his shroud, and twenty-four yards of black velvet to cover his coffin."

—

Exsection of the Head of the Humerus.—This operation was performed in June last by Prof. Geo. C. Blackman, in the case of a lad 5 years of age, admitted into the Commercial Hospital, with caries of the head and upper extremity of the humerus. The wound healed without any untoward symptom, and the lad is now in good health and has a pretty good use of the limb. *Western Lancet*, Aug., 1857.

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Gelseminum Sempervirens in Gonorrhœa.—Dr. John Douglas, of Chester District, S. C., states (*Charleston Med. Journ.*, July, 1857), that "about thirty years ago, I was called on, in my office, by a young man who had been suffering several months with improperly treated gonorrhœa. One of my pupils begged me to give the case to him, observing that he could cure the most obstinate case in a few days with the root of yellow jessamine. A small handful of the root was put into a common junk bottle of whiskey, and the patient ordered, in a day or two, to take a tablespoonful of this tincture night

and morning. He took but a few doses before he became much alarmed, and called on me, stating that the medicine had destroyed his vision. The symptoms he described correspond precisely with those mentioned by Dr. M. Every symptom of gonorrhœa had disappeared, and the cure was permanent. Since that time I have treated many cases of the same character in a similar manner with uniform and speedy success."

My experience with the medicine is not sufficient to determine whether it is absolutely necessary that the patient should be fully narcotized, but such was the condition in every case which I treated. I have no doubt but a more protracted use in smaller doses would answer the purpose.

—

Preservation of Vaccine Matter by Solution in Glycerine.—The Chicago correspondent of the *Peninsular Journal of Medicine*, states that Dr. Andrews, of Chicago, has made some experiments in the preservation of vaccine virus by solution in glycerine, using the solution instead of the solid matter for vaccination.

In Dr. Andrews' experiment, the vaccine matter was kept in solution two or three months of warm weather, at the end of which time seven cases were vaccinated with it, without a single failure. The scab broken into three or four pieces is thrown into a little glycerine, and occasionally shaken. It will slowly dissolve without further care. Dr. Johnson has repeated Dr. Andrews' experiments with success.

—

Woman with four Mammæ.—An example of this occurred lately in the Charity Hospital. The supplementary nipples were situated about two inches below the others. The patient had had seven children and was accustomed to suckle from three of the nipples. All furnished milk equally well, but the right supplementary nipple was small.—*N. O. Med. News and Hosp. Gazette*, Sept. 1857.

[M. Roberts (see No. for Aug., 1828, p. 412), records two cases, a mother and daughter, in which each had three mammæ. In one the third mamma was in the groin and served to suckle several children.]

Philadelphia Hospital, Blockley.—At a meeting of the Guardians of the Poor, held on the 8th of June last, Dr. James McClintock was elected Chief Resident Physician to this Institution. This appointment has been regarded by the profession of Philadelphia as a gross insult to them, and has been indignantly denounced both by the College of Physicians of Philadelphia and by the Philadelphia County Medical Society.

Immediately after the appointment of Dr. McClintock, all but one of the Resident Physicians of the Blockley Hospital, and all the members of the consulting medical, surgical and obstetrical staff resigned.

Of Dr. McClintock, we are unable to speak from personal acquaintance; but the following episode in his history as it is related by one who claims "*long-time friendship*"[1] with him, may be deemed authentic, and may serve to enlighten the reader as to his career:—

"It is now only three or four years since Dr. James McClintock, of Philadelphia, in an evil hour, consented, for a consideration of some $5,000, to furnish a series of recipes for 'Family Medicines,' and to allow his name and titles to accompany their announcement, in the usual style of nostrum venders, he and his heirs retaining an interest in the sales. The moneyed parties to this new phase of quackery were understood to be Thos. McElrath, Esq., of the *N. Y. Tribune*, and Wm. E. Burton, Esq., of the theatre, both of whom entered into this speculation with as high hopes as they ever made investments in any other 'fancy stocks;' and by dint of columns of advertisements, and editorial puffs from all the presses known to be in the market for hire, they rivalled Brandreth, Moffet, Jayne, Ayres, and even Hunter! in their advertising patronage to corruptible newspapers.

[1] Dr. D. Meredith Reese.

* * * "The bubble soon burst, and the 'Family Medicines' were a drug in the market, with no purchasers, either by wholesale or retail, but not until, it is said, some seventy thousand dollars had been sunk in the wild experiment. Meanwhile Dr. McC. was displaced from the American Medical Association for his offence against the code of ethics, and hence suffered a worse than pecuniary loss."

After announcing Dr McC.'s appointment, his friend, from whom we quote, adds: "Simultaneously he has renounced and repudiated all association with, or interest in, the business of nostrum vending, and has published, in the *Philadelphia Med. and Surg. Journal*, all his famous celebrated recipes in detail, *pro bono publico*."—*American Medical Gazette*, July, 1857.

We have in common, we believe, with every right minded physician, felt indignant at the appointment of Dr. McClintock, but we cannot say that it has either surprised or mortified us. It is one of the evils resulting from our social system to throw the governing power of many of our eleemosynary institutions, in which philanthropy and science should be the sole ruling influences, into the hands of mere politicians. Such are most of the managers of our Blockley Hospital. Elected solely on political grounds, they are incapable of any liberal or genuine philanthropic sentiments, and are utterly unable to appreciate the high and disinterested principles which govern the medical profession. That such persons should have been induced to elect to a high and responsible medical position an individual whose name had been stricken from the roll of membership of the American Medical Association for conduct considered by that illustrious body eminently derogatory to professional character, need not therefore excite surprise. Connected, however, with this subject is a circumstance which we must confess to have deeply mortified us. It is that a member of the profession, one unhappily, at the last meeting of this same American Medical Association, elevated to the honourable office of one of its Vice-Presidents, should have exerted his influence to promote the election of Dr. McClintock, and even have made use of that title to give force to his recommendation.

That we may do full justice to Dr. Reese, we subjoin his attempted justification:—

"Dr. James McClintock has been appointed, by the Guardians of the Poor of Philadelphia, Resident Physician-in-chief at the Blockley Hospital of that city. We are sorry to learn that the Assistant Physicians have resigned in a body, alleging as a reason the late exclusion of Dr. McC. from the American Medical Association, for his amalgamation with nostrum venders. They seem oblivious to the fact that this appointment was not made, nor even recommended by anybody, until after he had made a public recantation of the error of his life, and a disclaimer of all participation in the medical heresy for which he had been censured by his peers and condemned by the profession. But for this assurance, our signature advising his appointment could never have been obtained, much less would we have testified to his qualifications as a physician and a surgeon, fitting him for the place. But learning by authority that he had made all the atonement in his power for his violation of the Code of Ethics, and being assured that he would 'go and sin no more,' our resentment for his temporary apostasy ceased, and the recollection of what he has been as a medical teacher and practitioner, revived the long-time friendship which his error had broken, and we could not withhold our forgiveness of the repentant wanderer, consistent with our reverence for the 'higher law.'"—*American Medical Gazette*, July 1857.

Comment on this specimen of sophistry and cant is wholly unnecessary. We submit it to the profession with confidence as to their verdict.

As to our National Association, it will doubtless at its next meeting adopt some measure to wipe out the stain which the act of its Vice-President has inflicted on its honour.

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UNIVERSITY OF PENNSYLVANIA—MEDICAL DEPARTMENT.

NINETY-SECOND SESSION (1857–58).

The Lectures will commence on Monday, October 12, and continue until the middle of March.

ROBERT HARE, M. D., Emeritus Professor of Chemistry.
WILLIAM GIBSON, M. D., Emeritus Professor of Surgery.

SAMUEL JACKSON, M. D.,	Professor of Institutes of Medicine.
GEORGE B. WOOD, M. D.,	Professor of Theory and Practice of Medicine.
HUGH L. HODGE, M. D.,	Professor of Obstetrics and the Diseases of Women and Children.
JOSEPH CARSON, M. D.,	Professor of Materia Medica and Pharmacy.
ROBERT E. ROGERS, M. D.,	Professor of Chemistry.
JOSEPH LEIDY, M. D.,	Professor of Anatomy.
HENRY H. SMITH, M. D.,	Professor of Surgery.

WILLIAM HUNT, M. D., Demonstrator of Anatomy.

Clinical Instruction is given at the Pennsylvania Hospital, and at the Philadelphia Hospital.

Clinical instruction is also given, throughout the Session, in the Medical Hall, by the Professors.

The Dissecting Rooms, under the superintendence of the Professor of Anatomy and the Demonstrator, are open after the middle of September.

Fees for the Lectures (each Professor $15)	$105
Matriculation Fee (paid only once)	5
Graduation Fee	30

R. E. ROGERS, M. D., *Dean of the Medical Faculty, University Building.*

F. B. DICK, *Janitor, University Building.*

UNIVERSITY OF LOUISIANA—MEDICAL DEPARTMENT.

The Annual Course of Lectures in this department will commence on Monday, November 16, 1857, and will terminate in the ensuing March.

JAMES JONES, M. D.,	Professor of Practice of Medicine.
J. L. RIDDELL, M. D ,	Professor of Chemistry.
WARREN STONE, M. D.,	Professor of Surgery.
A. H. CENAS, M. D.,	Professor of Obstetrics.
GUSTAVUS A. NOTT, M. D.,	Professor of Materia Medica.
JOSIAH C. NOTT, M. D.,	Professor of Anatomy.
THOMAS HUNT, M. D.,	Professor of Physiology and Pathology.
GILBERT S. VANCE, M. D.,	Demonstrator of Anatomy.

The rooms for Dissecting will be open on the second Monday in October.

The Faculty are Visiting Physicians and Surgeons of the Charity Hospital.

The Students accompany the Professors in their visits, and, free of expense, enjoy extraordinary practical advantages.

Preliminary to the Course, Lectures will be delivered daily in the amphitheatre of the Hospital, from the 1st of October, on Clinical Medicine and Surgery, and other subjects, without any charge to students.

THOMAS HUNT, M. D., *Dean.*

The Administrators of the Charity Hospital elect annually, in April, fourteen Resident Students, who are maintained by the Institution.

JEFFERSON MEDICAL COLLEGE.

The Course of Lectures will commence on Monday, the 12th of October, and continue until the 1st of March.

THOS. D. MÜTTER, M. D., Emeritus Professor of Surgery.
ROBERT M. HUSTON, M. D., Emeritus Professor of Materia Medica and General Therapeutics.

Institutes of Medicine, etc., ...	By Prof. ROBLEY DUNGLISON, M. D.
General, Descriptive and Surgical Anatomy, .	" JOSEPH PANCOAST, M. D.
Practice of Medicine, ...	" J. K. MITCHELL, M. D.
Obstetrics and Diseases of Women and Children,	" CHARLES D. MEIGS, M. D.
Chemistry, ...	" FRANKLIN BACHE, M. D.
Institutes and Practice of Surgery, . .	" SAMUEL D. GROSS, M. D.
Materia Medica and General Therapeutics, .	" THOMAS D. MITCHELL, M. D.
Demonstrator of Anatomy, . .	ELLERSLIE WALLACE, M. D.

Clinics will be held regularly during the month of September; and every Wednesday and Saturday in the month of October, and during the course, Medical and Surgical cases will be investigated, prescribed for, and lectured on before the Class. During the year ending March the first, 1857, about *sixteen hundred* cases were treated, and about *two hundred* operations were performed; amongst them many major operations—as amputation of the thigh and leg, extirpation of the upper jaw, mammæ, &c., lithotomy, trephining, resection of the elbow-joint, and ligation of the external iliac artery.

The lectures are so arranged as to permit the student to attend the medical and surgical practice and lectures at the Pennsylvania Hospital.

On and after the 1st of October, the dissecting rooms will be open, under the direction of the Professor of Anatomy and the Demonstrator.

FEES:

Matriculation, which is paid only once, ...	$ 5
To each Member of the Faculty $15, ...	105
Graduation, ...	30

ROBLEY DUNGLISON, M. D.,
Dean of the Faculty.

KENTUCKY SCHOOL OF MEDICINE,

LOUISVILLE, KY.

The regular Session of this Institution commences on the first Monday in November, and continues till the 1st of March. The usual preliminary course will be given in October, at the College and Hospital.

FACULTY.

B. W. DUDLEY, M. D.,	Emeritus Professor of Anatomy and Surgery.
HENRY M. BULLITT, M. D.,	Professor of Theory and Practice of Medicine.
JOHN HARDIN, M. D.,	Professor of Obstetrics and Clinical Medicine.
CHARLES W. WRIGHT, M. D.,	Professor of Medical Chemistry.
MIDDLETON GOLDSMITH, M. D.,	Professor of the Principles and Practice of Surgery.
N. B. MARSHALL, M. D.,	Professor of Materia Medica and Therapeutics.
W. D. STERMAN, M. D.,	Professor of Special Anatomy.
GEO. W. BAYLESS, M. D.,	Professor of Physiology and Pathological Anatomy.
D. CUMMINGS, M. D.,	Demonstrator of Anatomy.

Fees for the entire course, $105. Matriculation Fee, $5. Graduation Fee, $25. Dissecting Ticket, $10.

Regular Clinical Lectures on Medicine and Surgery at the Hospital twice weekly during the session. Clinical Lectures at the College every Saturday afternoon.

For further information, address N. B. MARSHALL, *Dean*,
LOUISVILLE, KY., July 10, 1857. *College Buildings, cor. Green and Fifth Sts.*

PENNSYLVANIA COLLEGE—MEDICAL DEPARTMENT.

SESSION OF 1857–58.

The regular Course of Lectures will commence on Monday, October 12th, and will be continued until the 1st of March.

FACULTY.

DAVID GILBERT, M. D.,	Professor of Obstetrics and Diseases of Women and Children.
ALFRED STILLÉ, M. D.,	Professor of Theory and Practice of Medicine.
JOHN NEILL, M. D.,	Professor of Surgery.
JOHN J. REESE, M. D.,	Professor of Medical Chemistry.
JOHN B. BIDDLE, M. D.,	Professor of Therapeutics and Materia Medica.
FRANCIS G. SMITH, M. D.,	Professor of Institutes of Medicine.
T. G. RICHARDSON, M. D.,	Professor of General and Special Anatomy.

H. W. DE SAUSSURE FORD, M. D.,
J. FRANK BELL, M. D., } Demonstrators of Anatomy.

The Students of Pennsylvania College—*both first course and second course*—will be furnished *gratuitously* with the ticket to the Pennsylvania Hospital. A Clinic will also be held at the College, every Wednesday and Saturday morning throughout the session.

FEES.

For the entire Course of Lectures	$105 00
Matriculation (paid once only)	5 00
Graduation	30 00

The Dissecting Rooms will be opened in September, under the direction of the Professor of Anatomy.

Preliminary Lectures will be delivered during the fortnight preceding the opening of the session.

FRANCIS G. SMITH, M. D., *Registrar*,
July, 1857. *No.* 1504 *Walnut Street, Philadelphia.*

OGLETHORPE MEDICAL COLLEGE,

SAVANNAH, GEORGIA.

The regular course of Lectures in this Institution will commence on Monday, November 2, and continue four months. A preliminary course will commence on the 20th October.

The requirements for graduation are similar to those of other Medical Colleges in the United States.

Students will have ample clinical instruction, and surgical operations are always performed before the class when practicable.

A very liberal Beneficiary foundation is established in the College.

Good board may be had at from $3 50 to $5 00 per week.

FACULTY.

HOLMES STEELE, M. D., Dean,	Professor of Obstetrics and Diseases of Women and Children.
J. S. MOREL, M. D.,	Professor of Anatomy.
H. L. BYRD, M. D.,	Professor of Principles and Practice of Medicine.
J. W. BENSON, M. D.,	Professor of Principles and Practice of Surgery.
J. R. SMITH, M. D.,	Professor of Physiology and General Pathology.
IRA E. DUPREE, M. D.,	Professor of Materia Medica and Medical Jurisprudence.
JULES LE HARDY, M. D.,	Professor of Chemistry and Pharmacy.
HUGH A. BLAIR, M. D.,	Demonstrator of Anatomy.

FEES.

For the full course	$105 00
Demonstrator	10 00
Matriculation	5 00
City Hospital (optional)	5 00
Diploma	30 00

PHILADELPHIA COLLEGE OF MEDICINE.

FIFTH STREET, BELOW WALNUT.

SESSION 1857-8.

FACULTY.

ALFRED T. KING, M. D., Emeritus Professor of Practice of Medicine.

B. HOWARD RAND, M. D., Professor of Medical Chemistry.
HENRY HARTSHORNE, M. D., " Practice of Medicine.
LEWIS D. HARLOW, M. D., " Obstetrics, &c.
WILLIAM S. HALSEY, M. D., " Surgery.
WM. HEMBEL TAGGART M. D., " Materia Medica.
JAMES AITKEN MEIGS, M. D., " Institutes of Medicine.
W. H. GOBRECHT, M. D., " Anatomy.

WILLIAM BRADLEY, M. D., Demonstrator of Anatomy.

FEES: Matriculation, $5; one Full Course, $100; Perpetual Ticket, $150; Graduation, $30; Practical Anatomy, $10; Material for Dissection, *free*. Second course Students are furnished with the Hospital Ticket *without charge*. Lectures will commence early in October, and terminate in March. Hereafter this College will have but *one* Commencement annually; the Summer Lectures being made supplementary, only, to the Winter Course.

For further information, address B. HOWARD RAND, M. D., *Dean*.

MEDICAL COLLEGE OF VIRGINIA.

SESSION OF 1857-'58.

The regular Course of Lectures will commence on the 1st of October, and continue until the 1st of March.

CHARLES BELL GIBSON, M. D., Professor of Surgery and Surgical Anatomy.
DAVID H. TUCKER, M. D., Professor of Theory and Practice of Medicine.
MARTIN P. SCOTT, M. D., Professor of Chemistry and Pharmacy.
BEVERLEY R. WELLFORD, M. D., Professor of Materia Medica and Therapeutics.
ARTHUR E. PETICOLAS, M. D., Professor of Anatomy.
LEVIN S. JOYNES, M. D., Professor of Institutes of Medicine and Medical Jurisprudence.
JAMES H. CONWAY, M. D. Professor of Obstetrics and Diseases of Women and Children.
MARION HOWARD, M. D., Demonstrator of Anatomy.

The study of Practical Anatomy may be prosecuted with the most ample facilities, and at a very trifling expense.

The Infirmary, under the same roof with the College, is at all times well filled with Medical and Surgical cases, and furnishes peculiar advantages for Clinical Instruction. Many Surgical Operations are performed in the presence of the Class, and the Students, being daily admitted to the wards, enjoy, under the guidance of the Professors, unusual opportunities for becoming familiar, not only with the symptoms and diagnosis of disease, but with its daily progress and treatment. Students also enjoy the Clinical advantages afforded by the Richmond Almshouse.

FEES.

For the entire Course of Lectures	$105
Practical Anatomy	10
Matriculation Fee	5
Graduation Fee	25

L. S. JOYNES, M. D.,
April, 1857. *Dean of the Faculty.*

FISKE MEDICAL PRIZE QUESTION.

THE TRUSTEES OF THE FISKE FUND,

AT THE

ANNUAL MEETING OF THE RHODE ISLAND MEDICAL SOCIETY,

Held at Providence, June 3, 1857, announced that the premium of One Hundred Dollars offered by them in 1857, for the best dissertation on the following subject:—

"WHAT ARE THE CAUSES AND NATURE OF THAT DISEASE INCIDENT TO PREGNANCY AND LACTATION, CHARACTERIZED BY INFLAMMATION AND ULCERATION OF THE MOUTH AND FAUCES, USUALLY ACCOMPANIED BY ANOREXIA, EMACIATION AND DIARRHŒA, AND WHAT IS THE BEST MODE OF TREATMENT?" has been awarded to the author of the dissertation bearing this motto—

"Wheat from the fields of science, and cockles from my own farm."

And upon breaking the seal of the accompanying packet, they learned that the successful competitor was DAVID HUTCHINSON, M. D., of Mooresville, Morgan County, Indiana.

They propose the following subjects for 1858:—

I. THE EFFECTS OF THE USE OF ALCOHOLIC LIQUORS ON TUBERCULAR DISEASE, OR IN CONSTITUTIONS PREDISPOSED TO SUCH DISEASE.

To be supported by facts presented as far as possible in a statistical form.

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Every competitor for a premium is expected to conform to the following regulations, viz:—

To forward to the Secretary of the Trustees, on or before the first day of May, 1858, free of all expense, a copy of his dissertation, with a motto written thereupon, and also accompanying a sealed packet, having the same motto inscribed upon the outside, and his name and place of residence within.

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BOYLSTON MEDICAL PRIZE QUESTIONS.

The Boylston Medical Committee, appointed by the President and Fellows of Harvard University, consists of the following Physicians:—

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John Jeffries, M. D.	D. H. Storer, M. D.
S. D. Townsend, M. D.	Charles G. Putnam, M. D.
J. B. S. Jackson, M. D.	Morrill Wyman, M. D.

Henry J. Bigelow, M. D.

At the Annual Meeting of the Committee, on Wednesday, August 5, 1857, a premium of Sixty Dollars, or a gold medal of that value, was awarded to Wm. W. Morland, M. D., of Boston, for a dissertation on the following question:—

"*The Pathology and Treatment of the Diseases of the Urinary Organs.*"

The other Boylston Premium of the same value, was awarded to Ephraim Cutter, M. D., of Woburn, for a dissertation on

"*Under what circumstances do the usual Signs furnished by Auscultation and Percussion prove fallacious?*"

The questions for 1858 are:—

1. *Spermatorrhœa—its Causes, Consequences, and Treatment.*

2. *Human Parasites, Animal and Vegetable, their Anatomy, Development, Natural History, and Treatment.*

Dissertations on these subjects must be transmitted, post paid, to Edward Reynolds, M. D., *on* or *before* the *first Wednesday* of April, 1858.

The following questions are proposed for 1859:—

1. *New and useful views upon any subject in Medicine or Surgery.*

2. *Tubercle—its Pathology, and especially its Relation to Inflammation.*

Dissertations on these subjects must be transmitted as above, *on* or *before* the *first Wednesday* of April, 1859.

The author of the best dissertation considered worthy of a premium, on either of the subjects presented for 1858, will be entitled to a premium of *one hundred and twenty dollars*, or a gold medal of that value, at his option.

The author of the best dissertation considered worthy of a premium on either of the subjects presented for 1859, will be entitled to a premium of *sixty dollars*, or a gold medal of that value, at his option.

Each dissertation must be accompanied by a sealed packet, on which shall be written some device or sentence, and within shall be inclosed the author's name and residence. The same device or sentence is to be written on the dissertation to which the packet is attached.

The writer of each dissertation is expected to transmit his communication to the Chairman of the Committee, in a legible handwriting, within the time specified.

All unsuccessful dissertations are deposited with the Secretary, from whom they may be obtained, with the sealed packet unopened, if called for within one year after they have been received.

By an order adopted in 1826, the Secretary was directed to publish annually the following votes:—

1st. That the Board do not consider themselves as approving the doctrines contained in any of the dissertations to which premiums may be adjudged.

2d. That in case of publication of a successful dissertation, the author be considered as bound to print the above vote in connection therewith.

D. HUMPHREYS STORER,
Secretary.

Boston, Aug. 7, 1857.

TRANSYLVANIA UNIVERSITY—MEDICAL DEPARTMENT.

The Forty-First Session will commence on the First Monday in November, 1857, and will continue four months, under the direction of the following Faculty, viz:—

Benjamin W. Dudley, M. D.,	Emeritus Professor of Surgery.
Robert Peter, M. D.,	Professor of Chemistry and Pharmacy.
James M. Bush, M. D.,	Professor of Anatomy.
William S. Chipley, M. D.,	Professor of Theory and Practice of Medicine.
Ethelbert L. Dudley, M.D.,	Professor of Principles and Practice of Surgery.
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ROBERT PETER, M. D.,
Lexington, Ky., July 14, 1857. *Dean, &c.*

UNIVERSITY OF NASHVILLE—MEDICAL DEPARTMENT.

SESSION OF 1857-58.

The Seventh Annual Course of Lectures in this Institution will commence on Monday, the 2d of November next, and continue till the first of the ensuing March.

Thomas R. Jennings, M. D.,	Professor of Anatomy.
J. Berrien Lindsley, M. D.,	Professor of Chemistry and Pharmacy.
C. K. Winston, M. D.,	Professor of Materia Medica and Med. Jurisprudence.
A. H. Buchanan, M. D.,	Professor of Surgical Anatomy and Physiology.
John M. Watson, M. D.,	Professor of Obstetrics and the Diseases of Women and Children.
Paul F. Eve, M. D.,	Professor of Principles and Practice of Surgery.
W. K. Bowling, M. D.,	Professor of Institutes and Practice of Medicine.
William T. Briggs, M. D.,	Adjunct Professor and Demonstrator of Anatomy.

The Anatomical Rooms will be opened for students on the first Monday in October (the 5th).

A *Preliminary Course* of Lectures, free to all students, will be given by the Professors, commencing also on the first Monday of October.

The Tennessee State Hospital, under the direction of the Faculty, is open to the Class free of charge.

A Clinique has been established in connection with the University, at which operations are performed, and cases prescribed for, and lectured upon, in the presence of the class.

Amount of Fees for Lectures is $105. Matriculation Fee (paid once only), $5. Practical Anatomy, $10. Graduation Fee, $25.

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Nashville, Tenn., July 16, 1857. *Dean of the Faculty.*

MEDICAL COLLEGE OF GEORGIA,

AT AUGUSTA.

The Twenty-Sixth Course of Lectures in this Institution, will commence on Monday, the 2d November next.

G. M. Newton, M. D.,	Emeritus Professor of Anatomy.
H. F. Campbell, M. D.,	Anatomy.
L. A. Dugas, M. D.,	Surgery.
L. D. Ford, M. D.,	Institutes and Practice of Medicine.
I. P. Garvin, M. D.,	Materia Medica, Therapeutics and Medical Jurisprudence.
J. A. Eve, M. D.,	Obstetrics and Diseases of Women and Infants.
H. V. M. Miller, M. D.,	Physiology and Pathological Anatomy.
Alex. Means, M. D.,	Chemistry and Pharmacy.
Robt. Campbell, M. D.,	Demonstrator of Anatomy.
S. B. Simmons, M. D.,	Assistant Demonstrator of Anatomy.

Clinical Lectures will be delivered in the City Hospital and at the Jackson Street Hospital.

Fee for whole Course, $105.

Matriculation Ticket (to be taken once), $5.

For further particulars, apply to any member of the Faculty, or to

I. P. GARVIN, *Dean.*

MEMPHIS MEDICAL COLLEGE.

The Regular Lectures of this College will commence on Monday, the 2d of November, and will continue four months.

FACULTY.

John Millington, M. D.,	Professor of Chemistry and Toxicology.
Ayres P. Merrill, M. D.,	Professor of Principles and Practice of Medicine.
Lewis Shanks, M. D.,	Professor of Obstetrics and Diseases of Women and Children.
Arthur K. Taylor, M. D.,	Professor of Anatomy.
Howel R. Robards, M. D.,	Professor of Surgery.
C. B Guthrie, M. D.,	Professor of Materia Medica and Pharmacy.
Daniel F. Wright, M. D.,	Professor of Physiology and Pathology.

George F. Jones, M. D., Demonstrator of Anatomy.
J. F. Marable, M. D., Curator of the Museum.

L. SHANKS, M. D., *Dean of the Faculty.*

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A Preliminary Course of Lectures, free to all students, will be given by each Professor, commencing on Monday the 12th of October, and will be continued until the commencement of the regular course.

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The Memphis Charity Hospital is open for the visits of students, and will be visited regularly by one of the Faculty. The College Dispensary is open every morning; when about 1,000 patients are exhibited and prescribed for before the class.

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